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OF THE
American Schools of Oriental Research
JERUSALEM BAGHDAD

SUPPLEMENTARY STUDIES
Nos. 13–14

THE QUMRÂN (DEAD SEA) SCROLLS AND PALAEOGRAPHY

By
SOLOMON A. BIRNBAUM

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PUBLISHED BY THE
SCHOOLS OF ORIENTAL RESEARCH
YALE STATION, NEW HAVEN, CONN.)
1952

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EDITORIAL ANNOUNCEMENT

We are glad to be able to present Dr. S. A. Birnbaum's monograph on *The Qumrân (Dead Sea) Scrolls and Palaeography* in this series; it fills a gap in the extensive literature which has arisen around the extraordinary discovery of the Qumrân Scrolls. One might have thought that the preliminary accounts of the palaeographic data would alone have settled the question of their date, quite aside from direct archaeological evidence, but experience has demonstrated the contrary. Many leading philologians in this country and abroad seem totally unable to appraise the merit of palaeographical arguments, and one of the criticisms being leveled at the protagonists of a date before A. D. 70 for the *latest* items from the original find and of a date before the Christian era for the three scrolls published by the American Schools of Oriental Research, is that we have not answered our opponents' contentions. In this study Birnbaum has replied most effectively to the attacks made by Kahle, Teicher, Lehmann, Lacheman, and others. In this perfectly fantastic chapter of scholarly obscurantism (since the problem is essentially not one of opinion but one of factual evidence), polemics has its necessary place. Incidentally, the careful reader will receive an excellent training in Hebrew palaeographic method from the study of this monograph.

The present issue of *Supplementary Studies* should be followed in a few weeks by O. R. Sellers' long-awaited monograph, *A Roman-Byzantine Burial Cave in Northern Palestine.*

The following numbers of the *Supplementary Studies* are now available; Nos. 1 and 4 are out of print:

Nos. 2/3. *The Legend of King Keret: A Canaanite Epic of the Bronze Age.* By H. L. Ginsberg (1946, 50 pp., 2 plates, $1.25). This study has not been antiquated by the progress of research and it contains the only up-to-date philological commentary on the text.

Nos. 5/6. *The Ceramic Vocabulary of the Old Testament.* By James L. Kelso (1948, 48 pp., $1.25). Here we have the best treatment of the pottery of the Bible, on the basis of archaeological study of the material with an analysis of the biblical terminology.

Nos. 7/9. *The Early Arabian Necropolis of Ain Jawan: A Pre-Islamic and Early Islamic Site on the Persian Gulf.* By Richard LeB. Bowen, Jr. (1950, 70 pp., with 24 cuts in the text, $1.75 [$2.00 in boards]). The first publication of the results of excavation in Sa'ûdi Arabia, with chronological control by leading experts.

Nos. 10/12. *The Dead Sea Manual of Discipline: Translation and Notes.* By William Hugh Brownlee (1951, 60 pp., $2.00). With the aid of a number of the foremost specialists on the Scrolls, Professor Brownlee has given a very careful interpretation of this extraordinary historical document, which represents the beliefs and practice of the Essenes in the first century B. C.

Nos. 13/14. *The Qumrân (Dead Sea) Scrolls and Palaeography.* By S. A. Birnbaum (1952, $1.50). See above for a brief characterization.

All the above studies may be ordered postpaid from the American Schools of Oriental Research, Drawer 93A, Yale Station, New Haven, Conn.

W. F. ALBRIGHT

October, 1952

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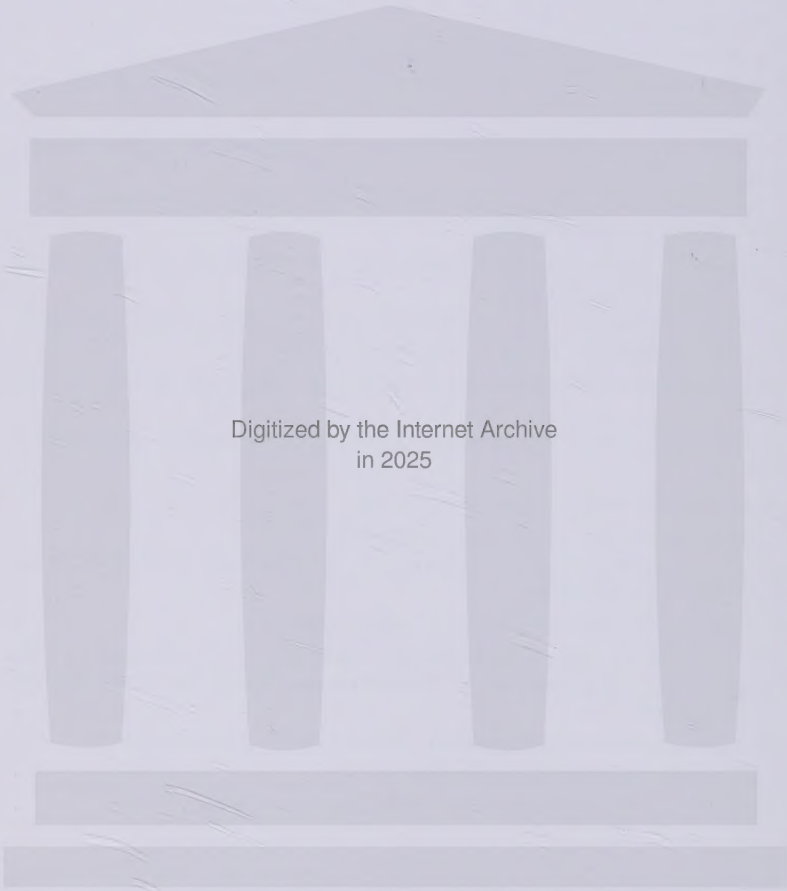

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PREFACE

The central problem occupying those engaged on research about the Cave Scrolls is still the question as to their dates. Much of the controversy revolves around the palaeographical aspect. It would therefore seem worth while to discuss the points which have been raised in the course of this debate.

In view of the inherent difficulties in establishing, by means of internal evidence, the historical context in which documents like the Cave Scrolls belong, the palaeographer may consider himself fortunate that he is not subject to the dangers of being misled by historical or textual theories. It makes no difference to him whether the WICKED PRIEST is King Jannaeus, King Aristobulus II, Paul or a medieval allegory; whether the *Kitti'im* are Macedonians or Romans, or in this Scroll the one and in that Scroll the other; whether the Masoretic Text was fixed in the second century B. C. E., the first century C. E., or at any other time; whether our Scrolls emanated from the Essenes or some other sect, or whether they are relics of an extensive literature which disappeared in the flames " of one of the greatest *auto-da-fés* known to world history." The palaeographer stands in no need of defending widely accepted hypotheses of long standing or pet theories of his own, nor of propounding important new ones. All he has to do is to sort out and classify the forms of the script, i. e., he is concerned only with objective data.

S. A. B.

November, 1951

ADDENDUM

News has now come through of the discoveries at Khirbet Qumrân and the caves in the Wâdi Murabba'ât. According to the findings of the archaeologists the settlement at Khirbet Qumrân existed during a period stretching from about the beginning of the 1st cent. C. E. to the year 70. A jar that was excavated there is said to be identical in type with those found with the Cave Scrolls. From this it would appear that the MSS. were deposited in the Cave during the period of the settlement.[1]

Thus those archaeologists who had formerly given the end of the 2nd cent. B. C. E. or in the early 1st cent. B. C. E. as their *terminus ad quem* for the depositing of the MSS., are now in full accord with the results arrived at on purely palaeographical grounds. For the script of some of the Scrolls (Isaiah *B*, War, and *Hodayot*[2]) dates from the Ossuaries period (late 1st cent. B. C. E. to 70 C. E.).

As regards the Murabba'ât Caves it has been found that they were in use over a period of many centuries, particularly during the 8th-7th cent. B. C. E. and the 2nd cent. C. E. Among the finds reported is a

Palaeo-Hebrew palimpsest, evidently belonging to the earlier period, and three documents from the 2nd cent. C. E. (a dated marriage deed of 124 C. E. and two letters from Bar Kochba—if that proves to be the identity of their sender, " Simeon ben Kosebah "). Both groups appear to have been found in the same cave. If that should prove correct it would be comparable with the discovery of the Scrolls and the Leviticus Fragments in one and the same cave. This might perhaps suggest to those who are sceptical about the early dating of the LevFgms that, like the Murabba'ât Cave, the Qumrân Cave contained deposits from various periods.

The script of the marriage deed and of the two letters will, no doubt, prove to be transitional between that of the latest Cave Scrolls and that of the Dura documents.* But what are we going to see in the MSS. reported to be " written in a type of cursive hitherto unknown and therefore very difficult to read " ?

May, 1952

[1] At any time between the beginning of the century and 70, if the Cave was used as a *genîza*, or between 66 and 70, if it was a cache—and that seems on the whole to be more likely. As a matter of fact, a combination of both is not impossible: part of the material might have been put aside for *genîza* and, while awaiting burial, might in an emergency have been hurriedly taken to the Cave together with the books which were being deposited there for safety.

[2] Cf. my article " The Date of the Hymns Scroll " (*PEQ*, October, 1952).

* According to word received from Père de Vaux, the script of the Hadrianic documents is " plus évoluée " than that of the Qumrân Scrolls. Since January of this year manuscript finds in the strip between the mountains of Judaea and the northwestern shore of the Dead Sea have been multiplying at accelerated speed. The latest cave is said on very good authority to be even richer than the first Qumrân Cave. Together with the thousands of fragments unearthed by foreign scholars and Bedu—and not forgetting the official records carved on the rolled-up copper strips—we have a fabulous wealth of material, mostly to be dated between 200 B. C. and A. D. 135 (W. F. A.).

I

A Matter of Presentation

In the course of a report on the palaeographical work done about the Cave Scrolls, O. Eissfeldt expressed the opinion that it is "on the right track."[1]

In his book on the Cave Scrolls,[2] P. Kahle refers to Eissfeldt's article but omits this essential statement. His mention of the article is embedded in an abundance of negatives: "... papyri and ostraca.... We have seen how uncertain their dating is. About the attempt ... to reach an objective date for the scrolls found in the cave, with the help of these documents which are not datable with certainty, Eissfeldt, for example, has reported at such length, that I can refer the reader to that. I am convinced that Lacheman is right when he points out that in this way one cannot arrive at a safe result, at least not for the time being."[3]

Anyone who has not read Eissfeldt's article would conclude from K.'s words that Eissfeldt had condemned the palaeographical method, when, on the contrary, what he said was that the palaeographical investigation regarding the Scrolls is "on the right track." A rather different matter.[4]

[1] "auf dem rechten Wege" ("Ansetzung der Rollen nach paläographischen Kriterien." *TLZ* 74 [1949]: 226-8).

[2] *Die hebräischen Handschriften aus der Höhle.* Stuttgart, 1951.

[3] "... Papyri und Ostraka ... Wir haben gesehen, wie unsicher ihre Datierung ist. Über den Versuch ... mit Hilfe dieser nicht sicher datierbaren Dokumente zu einem objektiven Datum für die in der Höhle gefundenen Rollen zu kommen, hat z.B. Eissfeldt so ausführlich berichtet, dass ich darauf verweisen kann. Ich bin davon überzeugt, dass Lacheman recht hat, wenn er darauf hinweist, dass man auf diesem Wege zu einem sichern Resultat nicht kommen kann, jedenfalls einstweilen nicht." (p. 22)

[4] Since writing these words I have come across a passage in Einar Brønno's article 'Zu den Theorien Paul Kahles von der Entstehung der tiberischen Grammatik' (*ZDMG* 100 (1950) [1951]: 521-65, which is a reply to Kahle's criticism of his book *Studien über hebräische Morphologie und Vokalismus auf Grund der Mercatischen Fragmente der Zweiten Kolumne der Hexapla des Origenes*, Leipzig, 1943). K. had said: "Brønno quite arbitrarily decides not only which of the Greek vowels have to be pronounced as short or long, but also where we have to pronounce them as indefinite vowels (*Murmelvokale*), corresponding to Tiberian Shwa!" (*The Cairo Geniza*, p. 232) To this Brønno rejoined: "This statement of Kahle's does not accord with the facts. If an authority like Kahle writes in such a way, then all those whose knowledge of my book is confined to Kahle's mention of it, must come away with the impression that I have made *arbitrary assertions*. The position, however, is that I have brought detailed evidence in support of all I have said, and this cannot be demolished by means of an exclamation mark." ("Diese Äusserung Kahles stimmt nicht mit den Tatsachen überein. Wenn eine Autorität wie Kahle so schreibt, muss jeder, der das Buch nur aus der Erwähnung Kahles kennt, die Auffassung erhalten, es handele sich um *willkürliche Behauptungen* meinerseits, während in Wirklichkeit von einer ausführlichen Dokumentation, die sich jedenfalls nicht durch ein Ausrufungszeichen vernichten lässt, die Rede ist.' *l.c.*, p. 534)—Another passage: "Instead of checking up carefully whether my results regarding Codex B are correct, Kahle tries to turn down the whole piece of work as worthless, by polemizing against views which I never put forward." ("Anstatt nachzuprüfen, ob meine Resultate hinsichtlich des Codex B zutreffen, versucht Kahle die ganze Arbeit als nichtig abzulehnen, indem er gegen Gesichtspunkte polemisiert, die ich niemals vorgebracht habe." *Ibid.*, p. 564.)

Why Not Palaeography?

Accepting Dupont-Sommer's date for the composition of the Habakkuk Commentary—shortly before 40 B. C. E.—K. expresses the view, based on textual evidence, that the actual writing of the Scroll must have taken place before the destruction of Jerusalem (p. 59), i. e., according to him, our copy originated somewhere within a period of approximately 110 years. As to Isaiah Scroll *A*—which would, naturally, be of very much greater importance for his particular studies—he allows himself a margin double as great, i. e., practically two and a half centuries: his *terminus a quo*, based on differences in text and spelling, is the Maccabaean Revolt, and his *terminus ad quem* the Fall of the Temple (p. 76).

A somewhat more precise date would be of great value for his own particular field of research. As he is not able, by means of the instruments at his disposal—that is, in the main, textual evidence—to arrive at one, it is a pity that he refuses the help of palaeography.

The Weight of Evidence

K. brackets the names of Trever and myself with that of Albright, thus reducing the weight of the palaeographical results arrived at by the three of us to one piece of evidence: "About the attempt made by Albright, and, following him, Trever and Birnbaum, to reach an objective date. . . ." [5]

This cannot be done, however. That my examination was quite independent of those made by the other two scholars, should at least have been clear to him from the fact that I did not utilise the Nash Papyrus. But he does not appear to have realised it even after I actually pointed it out.[6]

If he had, the circumstance that, independently of Albright and Trever, I arrived at similar results, should have given him food for thought: Could such a thing have been a mere coincidence?

Reliability of Basis

K.'s main criticism of the dating of the Cave Scrolls by way of palaeography is that the documents utilized for comparison are not "datable with certainty."

Of Albright's material he says "that none of the texts utilised [by him] is datable with certainty," [7] and of Trever's discussion, that "on the whole, [it] follows in the footsteps of Albright; but it betrays that he has not sufficient experience of his own in the field of palaeography." [8]

As to the criticism contained in the last half of this sentence, one wonders whether a scholar who in the course of his work has handled many MSS. is *ipso facto* in a position to pass judgment on palaeographical work. Experience with MSS. is not a substitute for specialised, systematic

[5] "Über den Versuch, den Albright, und in seinem Gefolge Trever und Birnbaum unternommen haben . . . zu einem objektiven Datum . . . zu kommen. . . ." (p. 22)

[6] BASOR 115 (1949): 20.

[7] "dass keiner der beigezogenen Texte mit Sicherheit zu datieren ist." (p. 5)

[8] "Trevers Ausführungen bewegen sich im grossen und ganzen auf den Bahnen Albrights; aber man merkt es ihnen an, dass er keine genügende eigene Erfahrung auf dem Gebiete der Paläographie hat." (p. 6)

palaeographical research. It is significant that in his whole book K. does not base a single argument on the forms of letters, which, after all, constitute the subject of palaeography proper.

Finally, he says: " But although Dr. Birnbaum's attempts at reaching a more precise dating of the Isaiah and Habakkuk Scrolls are made with greater knowledge of detail, one must remain suspicious, because, of the parallel texts utilised, not a single one is datable with certainty." [9] From which it would follow that the results arrived at on such a basis are useless.

The documents utilised by me *are*, however, " datable with certainty." To see that, it is only necessary to look up what has been published about them.[10] It is generally accepted that the Egyptian papyri and ostraca which I utilised in my comparison are of the 3rd cent. B. C. E., the Tobias inscription of between 183 and 175 B. C. E. (Vincent: *c.* 260 B. C. E.), the Gezer Boundaries of the first third of the 1st cent. B. C. E., the Ossuaries, including the Uzziah Tablet, of the hundred years ending with the fall of the Temple, some of them of the late part of the 1st cent. B. C. E., some of the early decades of the 1st cent. C. E., and the Queen Helena Graffito of 50-60 C. E.

This is the evidence with which K. seems to have omitted to acquaint himself. If, however, he did study it and arrived at the conclusion that the results of Clermont-Ganneau, Cowley, Lidzbarski, Littmann, Sachau, Albright, Dussaud, Sukenik, Vincent, etc. are wrong or very doubtful, he should have explained why he thought so and made some attempt at disproving them.

So much as to the " certainty " of the dates. And as to their precision: Is a date which is precise to within a decade (Helena), not very precise? Are not those dates which go to within a third of a century (Gezer, some ossuaries) or half a century (some ossuaries) rather precise? And cannot even those documents (Egyptian papyri and ostraca, some ossuaries) which are datable only to within a century be nonetheless used to very great advantage?

How does K. explain the fact that I arrived at dates that go very well with those he himself endorses or suggests on other grounds? After all, my results were not influenced by extra-palaeographical evidence, they were printed before the archaeologists had found the jar fragments and announced that these were of the late Hellenistic period, and before the various theories based on internal evidence and speaking for a pre-Christian date, had been published.

The Square Reconstruction

Discussing the Leviticus Fragments, K. speaks of my " attempt at reconstructing the column but in Square script (!)." [11] His exclamation

[9] " Aber auch gegenüber den mit grösserer Detailkenntnis ausgeführten Versuchen Dr. Birnbaums, auf paläographischem Wege zu einer genaueren Datierung der Jesaja-Rolle und der Habakkuk-Rolle zu kommen, muss man misstrauisch bleiben, weil von den herangezogenen Paralleltexten kein einziger mit Sicherheit zu datieren ist." (p. 6)

[10] See my bibliography in *JBL* 68 (1949): 161-2.

[11] " Rekonstruktionsversuch der Kolumne, aber in Quadratschrift (!)." (p. 62)

mark seems to imply: to use Square letters in reconstructing a Palaeo-Hebrew text is methodologically wrong.

He fails to tell his readers what I expressly said about this point: "The reconstruction has to be done in the original alphabet, of course." [12] In the face of these unambiguous words of mine it is hard to understand how he can imply that I failed to make my own reconstruction in the original characters.

The printed text is obviously not my original reconstruction in Palaeo-Hebrew characters but a transliteration of it, made for the convenience of the readers, many of whom would not have great fluency in reading Palaeo-Hebrew script.[13] K. himself substitutes Square letters for Samaritan ones.[14]

Palaeo-Hebrew as a Display Script

After saying that in the 1st cent. B. C.E. Bible MSS. were written in Square script, he continues: "But there can be no doubt that the Old-Hebrew script remained [after it had been discarded for the writing of Tora scrolls] also among the Jews as a kind of display script." [15]

K.'s words "There can be no doubt" show his statement to be based on an inference and not on actual evidence.

Is he by any chance referring to the script on the coins? Whatever might be the reason for the lettering used on them, there is a complete absence of evidence to suggest that it was employed also for ornamental purposes in MSS. written in Square letters. And MSS. are what we are concerned with here.[16]

Palaeo-Hebrew Bible Scrolls in the Christian Era

The last-quoted sentence goes on: "and that Bible MSS. in Old-Hebrew script existed among Jews still in the Christian era." [17] To prove this he quotes *mishna* 5 of Yadayim, ch. 4. Here we read: "The Aramaic [texts] in Ezra and in Daniel make the hands impure. Aramaic [Biblical text] which was written in Hebrew [language], or Hebrew [Biblical text] which was written in Aramaic [language], or in [Palaeo-]Hebrew script, do not make the hands impure. Impurity is caused only by writing [a Bible MS.] in Assyrian [script], on skin, and with ink." [18]

[12] BASOR 118 (1950): 22.

[13] Perhaps it would not have occurred to K. to make this criticism if my reconstruction had been published in printed type. As a matter of fact, the use of a drawing was an economy measure on the part of the Editor, owing to the expense involved in Hebrew type-setting.

[14] *Op. cit.*, p. 20.

[15] "Es ist aber ausser Frage, dass die althebräische Schrift auch bei den Juden noch lange Zeit als eine Art Antiqua im Gebrauche war. . . ." (p. 20)

[16] The use of the Palaeo-Hebrew letters for the Tetragrammaton in the Habakkuk Scroll and for the word *'l* in the Hodayot Scroll and *El* Fragment disproves the display script assertion. Cf. below p. 26.

[17] "und dass Bibelhandschriften in althebräischer Schrift bei den Juden noch in nachchristlicher Zeit vorhanden gewesen sind." (p. 20)

[18] *Trgwm šb'zr' wšbdny'l mṭm' 't hydym trgwm šktbw 'bryt w'bryt šktbw trgwm wktb 'bry 'ynw mṭm' 't hydym l'wlm 'ynw mṭm' 'd šyktbnw 'šwryt 'l h'wr wbdyw.*

He believes this *mishna* furnishes him with an actual date: "This statement, coming from the 2nd cent. C. E., shows that at the time the Jews had, apart from Bible MSS. in Square script . . . also Bible MSS. in Old-Hebrew script." [19]

The chronological inference is, however, wrong. The fact that the Mishna was codified in the 2nd cent. does not mean that every *mishna* in it belongs to this period—on the contrary, only a few would. Various collections of *mishnas* go back to the 1st cent. C. E., the *Mishna Rishona* to the 1st cent. B. C. E. And the *mishnas* themselves antedate the collections, of course.

On the basis of his fallacious inference K. goes on to say that the Leviticus Fragments need not necessarily be several centuries earlier than the Christian era, and might therefore be contemporaneous with the fragments in Square script.[20]

In order to reach that conclusion, his reasoning would presumably have been somewhat on these lines: Our *mishna* 5 shows that Bible scrolls in Palaeo-Hebrew script were still in existence in the 2nd cent. C. E. It may therefore be assumed that these scrolls were written not very long before that time, i. e., in the centuries immediately preceding the 2nd cent. C. E. In other words, the Jewish scribes between the 2nd cent. B. C. E. and the 1st cent. C. E. were producing not only MSS. in Square script but also some in Palaeo-Hebrew. That being the period of the Cave Scrolls, the presence of the Leviticus Fragments (Lev. Fgms.) among the Square script material would thus be accounted for. Hence there is no need to ascribe the Lev. Fgms. to a much earlier century.

The assumption on which all this hinges is, however, an arbitrary one. Even if some Palaeo-Hebrew scrolls did survive into the 2nd cent. C. E. it would be no more reasonable to ascribe them to the 2nd cent. B. C. E. —1st cent. C. E. (which would be in figures the time indicated by K. in general terms) than to the 4th or 5th cent. B. C. E.

On the contrary, a very simple reason militates against that assumption. At a time when, in K.'s own words about Yadayim 4, 5, "only texts written in Square script were considered sacred" (p. 21), new Tora scrolls would not have been written in a discarded script which would *ipso facto* have rendered them unusable.

That the 2nd cent. B. C. E. already belonged to the time when only Bible scrolls in Square letters were considered sacred seems to be disputed by no one. The Cave Scrolls, according to K., date from between the 2nd cent. B. C. E and the 1st cent. C. E. Hence it is impossible that

[19] "Diese aus dem zweiten nachchristlichen Jahrhundert stammende Feststellung zeigt, dass die Juden damals ausser Bibelhandschriften in Quadratschrift . . . auch solche in althebräischer Schrift . . . gehabt haben." (p. 21)

[20] "Daraus ergibt sich . . . dass von Juden stammende Fragmente der Tora in alter hebräischer Schrift nicht unbedingt mehrere Jahrhunderte vor unserer Zeitrechnung geschrieben sein müssen. . . . Sie mögen älter sein als die in Quadratschrift geschriebenen Fragmente; aber ein Zwang zu dieser Annahme besteht nicht. Wir müssen mit der Möglichkeit rechnen, dass sie aus derselben Zeit stammen wie jene." (p. 21)

during this period any Bible scrolls could have been newly written in Palaeo-Hebrew characters, and the Lev. Fgms. cannot therefore be contemporaneous with the Cave Scrolls. They must be older.

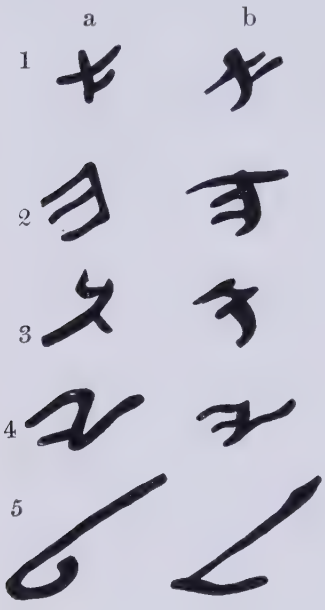

Fig. 1.
The Palaeo-Hebrew script in the Square script Scrolls contrasted with that of the Leviticus Fragments.
* Column *a*, line 1: Hodayot Scroll. 1* Lines 2-3: Hab. Scr. 2* Line 4: Hodayot Scr. 1* Line 5: *El* Fragment 3*. Column *b*: Leviticus Fragments 4*.

It is not even possible to place the Lev. Fgms. in the Cave Scroll period by ascribing them to the Samaritans of that time. For—quite apart from the question as to whether the letter forms in the Fgms. could belong to that period—their text, as K. himself has demonstrated, is not the one known to us as Samaritan.[21]

We have no reason to doubt that from the time when, centuries before the Christian era, Square became the script for Jewish Tora scrolls, no more were written in the discarded Palaeo-Hebrew alphabet.

The historical argument has been used here [22] only in order to show that K. has applied it incorrectly. But the safest way of showing that the Lev. Fgms. cannot have been written at the same time as the Cave Scrolls is by means of palaeography. We need only glance at Fig. 1 to see how much younger the forms in the Scrolls are than those in the Lev. Fgms. The detailed discussion in BASOR [23] and the charts in *VT* [24] demonstrate that the time interval between the two sets is approximately four centuries.

"Strange Precision"

In a sort of summary of my article on the Lev. Fgms., K. quotes what he calls my conclusion: "The Leviticus Fragments were part of a scroll written in a beautiful professional hand, about the second half of the fifth century B. C. E." This moves him to exclaim, "We ask ourselves in some surprise how he can arrive at this strangely precise date." [25] The explanation is to be found, he thinks, in this passage of mine: "That

[21] A kind fate has given us in the few words on our scraps a passage where the Jewish and Samaritan texts differ. The Lev. Fgms. have the Jewish form *'tkm h'rṣ* while the Samaritans read here *h'rṣ 'tkm*.
[22] It was not used at all in establishing the date in my article on the Lev. Fgms.
[23] 118 (1950): 25-7.
[24] 1 (1951): 107-8.
* Notes 1*-32*, indicated here and in the rest of the illustrations, are given on p. 51. They contain information as to where facsimiles from the MSS. utilized in the illustrations can be found. In a few cases where I do not know an available facsimile, reference is made to the original MSS.
[25] "Man fragt sich etwas erstaunt, wie er zu dieser seltsam genauen Datierung kommen kann." (p. 62)

is the period when the great change in the script of the Tora took place. Thus the scroll of which the Leviticus Fragments formed a part belonged to the final period when Palaeo-Hebrew was in use."

In other words, what K. suggests is that my date for the Lev. Fgms. is based not on palaeography but on an historical argument.

This is more than strange in the light of pp. 25-27 of my article, where my examination [26] of the script culminates in a conclusion as to its date—the result being reached by three lines of approach (paragraphs 2 to 4 of p. 27).

To his explanation he adds the following remark: "All the learning of the article has essentially the purpose of proving that a certain Talmudic opinion, as he understands it, is correct." [27] These words imply that I had my result ready before starting to work, and "cooked" my long, detailed and intricate palaeographical examination to fit a predetermined date! [28]

Codices

K. constructs a *terminus a quo* for the depositing of the Scrolls in the Cave from the fact that some of the papyrus fragments discovered there had writing on both sides. He declares: "Texts written on both sides are parts of codices" [29]—and in this simple way gets his *terminus*. For "manuscripts in codex form in the East can hardly be shown to have existed before the 2nd cent. C. E." [30]

It is not correct to say that writing on both sides occurs only in codices. Long before the time of codices the backs of discarded scrolls were written upon.

If it could be shown that the fragments in question contain a consecutive text on both sides, written by one and the same hand, and in the same direction, then it would be possible to say the fragments were parts of codices.

What, however, are the facts?

There are about 580 skin fragments from the Cave, and not one of them has writing on both sides. There are about 30 papyrus fragments, six of which have writing on both sides: Three are indistinct; the writing of the fourth is not identical on both sides: one side Greek, the other indistinct; the writing of the fifth is not identical on both sides, its script is indeterminate. These cases, although not conclusive, by no means

[26] "which looks very learned" ("sehr gelehrt aussehend"), he says (p. 62). Cf. with this expression what he writes about Zeitlin's "tone," "presumptuous tone" ("überheblich"), the "high and mighty way" ("von oben herab"), Z. speaks to other scholars (pp. 8 and 10). Cf. also K.'s quotation marks for *Wissenschaft* in note 28.

[27] "Die ganze Gelehrsamkeit des Artikels dient also im wesentlichen dazu, eine talmudische Anschauung, wie er sie versteht, als korrekt zu erweisen." (p. 63)

[28] At the end of this section, K. says that in view of such "Wissenschaft" on my part, it would suffice to point to Lacheman's criticism of "analogous attempts at dating." ("Es genügt gegenüber derartiger 'Wissenschaft' auf die Kritik hinzuweisen, die Ernest René Lacheman gegenüber analogen Datierungsversuchen geübt hat in seinem Artikel 'A Matter of Method in Hebrew Palaeography.'")

[29] "Doppelseitig beschriebene Texte sind Teile von *Codices*. . . ." (p. 58)

[30] "Handschriften in Codexform sind vor dem 2. nachchristlichen Jahrhundert im Osten kaum nachzuweisen" (*ibid.*).

favour the codex theory. The sixth fragment is definitely not from a codex because the writing runs in opposite directions on the two sides. According to this evidence,[31] the likelihood that these are the remains of papyrus codices seems to be exceedingly small.

Parchment

In the chapter on the Lev. Fgms. K. points to what he imagines is an inherent contradiction between my date and my description of the Fgms. as parchment: how could they be so old as I say, seeing that parchment is generally known to have been invented only in the Christian era? He asserts that I three times stress the fact of its being parchment, and underlines this by his manner of formulation.[32]

I have nowhere in my article discussed the question of the writing material (beyond mentioning it), much less stressed what the material is. K.'s words convey a quite misleading picture.

As to the parchment question:

K. did not know our *terminus AD quem* for the invention of parchment is 195 B. C. E., from which we have a dated document.[33] The quality of this piece is practically as good as that of any later first class Western vellum. (". . . une membrane si fine et si lisse qu'elle le cède de peu au velin des manuscrits les plus soignés").[34]

This fact would seem to suggest, or at least to make it appear not unlikely, that the development culminating in such a quality had been going on a considerable time before 195 B. C. E.[35]

The whole question is to a considerable extent one of terminology. As far as I know the various ancient kinds of writing skin have not yet been systematically and comprehensively examined and classified. As long as that is so, it might be preferable to use the general term skin (as I did, *e. g.*, in my article in *VT*).

The material of Isaiah Scroll *A* was thus described: "somewhat coarse parchment, or carefully prepared skins approaching the refinement of parchment. (Among the scrolls there is a considerable difference in the

[31] R. de Vaux, "La Grotte des manuscrits hébreux," *R B* 56 (1949): 597: "Nous en avons recueilli six cent." There are also some fragments in the Hebrew University. "Nous avons recueilli une trentaine de fragments sur papyrus; *six* sont écrits des deux côtés: l'un a de l'hébreu sur les deux faces, mais l'écriture du 'revers' est inversée; ce n'est donc sûrement pas un codex. Sur deux autres, la graphie n'est pas la même des deux côtés; l'un est une écriture indéterminée, l'autre est d'un côté en grec, de l'autre côté indistinct. Les trois fragments restant sont indistincts. . . . Les fragments sont d'ailleurs minuscules et ne contiennent que quelques lettres." R. de Vaux, in a letter to me, of February, 1951.

[32] "Dass diese so alt sein sollenden Fragmente auf *Pergament* geschrieben sind, das bekanntlich erst in nachchristlicher Zeit erfunden ist, wird von Birnbaum in seinem Artikel dreimal betont." (p. 63)

[33] F. Cumont, "Le plus ancien parchemin grec." *RP* 48 (1942): 97 ff. *Idem*, *Fouilles de Doura-Europos* (Paris, 1926): 281 *ff*.

[34] *Idem*, *Fouilles* . . . , p. 283.

[35] The pre-Christian origin of parchment disposes also of the basis of K.'s argument that the 4th cent. C. E. is the *terminus ad quem* for the depositing of the Scrolls in the Cave: "If the manuscripts had been deposited in the 4th cent. or later we would have to expect to find in the Cave, texts written on parchment." ("Wären die Handschriften im 4. Jahrhundert oder später in der Höhle geborgen worden, so müssten wir erwarten, auf Pergament geschriebene Texte in der Höhle zu finden." p. 58.)

degree of refinement and quality of the skins making it somewhat difficult to classify the material.)"[36] That of the Habakkuk Scroll is given as "soft leather," "the hair side, on which the writing was done, was carefully dressed and is smooth to the touch."[37]

The Leviticus Fragments, too, are written on the hair side.[38] Hence, in the terminology of the Talmud,[39] the material of the Cave MSS. is *'wr* "hide, skin" or *gwyl* (? "primitive kind of parchment").[40]

II

In a recent article,[1] Dr. J. L. Teicher condemns all the work on the origin of the Cave Scrolls hitherto published in the following terms: "All attempts that have been made hitherto to solve the problem of the origin of the Scrolls have one thing in common: they are based on an arbitrary selection from the total data supplied to us by the Scrolls themselves or by the circumstances of their discovery." (p. 78) He then quotes, by way of example, a few instances, selected from the writings of those who advocate a mediaeval date on literary and linguistic grounds. As to the "archaeologists and palaeographers who sponsor the pre-Christian date," he makes the unsubstantiated allegation that they "airily reject" all other evidence than their own.

In treating of the palaeographical aspect, T. deals to a not inconsiderable extent with my work. I do not know where I have rejected—"airily" or otherwise—any kind of evidence. On the contrary, in the first part of my lecture to the Palestine Exploration Fund in March 1950 (cf. *JBL* 70 [1951]: 227 *ff.*) I discussed the available extra-palaeographical evidence. And even in the principal part of my lecture—that dealing with palaeography (cf. *VT* 1 [1951]: 91 *ff.*)—I clearly stated that in examining the external evidence, to which the script belonged, a basis was being prepared for work on the rest of the evidence: "the natural method of procedure is to begin by examining all the external evidence and thus to create a basis upon which to continue further investigation." (*ibid.*, 92). That, in my humble opinion, is something very different from rejecting internal evidence as useless.

Shapes *In Abstracto*

T. characterises as "extremely perplexing" a method which "appears to be" based on the idea "that the shapes of letters considered *in abstracto*, as they appear to the eye, supply us with the criterion for fixing the date of the script" (p. 80).

[36] *DSS* 1: XIV.
[37] *Ibid.*, 1: XX.
[38] R. de Vaux, *RB* 56 (1949): 597.
[39] *J. Meg.* ed. Krot, fol. 71 d.
[40] Since writing these lines I have come across two learned articles by G. Zuntz, devoted to rectifying K.'s statements about parchment and codices, and the chronological conclusions he drew from them regarding the Cave Scrolls. See *TLZ* 76 (1951): 161-2 and 533-34.—Cf. also the article coll. 533-6, where an expert like W. Schubart shows that the writing materials of the Cave MSS. cannot help with the dating. That must be done, he says, by means of palaeography and textual criticism.
[1] "The Dead Sea Scrolls—Documents of the Jewish-Christian Sect of Ebionites," *JJS* 2 (1951): 67-99.

Now, the shapes of the letters, as they appear to the eye, have always been the basis of palaeographical study. This is not a novel method expressly created for the benefit of the Cave Scrolls. It is therefore not a methodological mistake to make "promiscuous use . . . of epigraphic material . . . and manuscripts" (p. 80): in the hands of an expert it is perfectly legitimate. Nobody disputes that the writing material exerts an influence, and that caution must therefore be exercised to distinguish between basic and accidental differences. Hence "the manner in which the scribe formed the letter"[2] (p. 80) is of chronological value if it depicts a change in the basic shape, i. e., in the ideal form in the mind of the writer. Palaeographers have always been aware that the style of inscriptions differs from that employed in MSS., but they have none the less used both promiscuously—when material was scarce—while exercising due care and discrimination.

Evolution

Next T. sets out to show that it is not legitimate for the palaeographer to base his work on developments in the forms of letters. He says: "The proposition that changes in the shape of a letter represent stages in the 'evolution' of the script is also problematic" (p. 81).

There is no problem here. There is no doubt that the changes in the shape of a letter do represent stages in its evolution. How is it possible to deny what experience confirms a thousand times over?

He goes on: "No 'law of nature' regulates the 'evolution' of the script in a given direction" (p. 81).

The causes which change the forms of letters are, in the last analysis, of a psychological and sociological nature. Hence the development of these forms actually is, in a way, governed by laws of nature, although not in the mechanical sense suggested by T. And as to the "given direction," the evolution does not take place in a direction which can be predicted. But that there *has* been an "evolutionary line" in writing can be seen by looking back: it is the observable chain of changes which have taken place, not the *a priori* figment which T. first imputes to the palaeographers and then calls "largely fictitious."

Why Do the Forms Change?

T. rounds off his disposal of the "evolution" of letter forms by explaining what, in his opinion, is the reason why they change: "The changes in the shape of letters which may have come about as a result of the inner development of the art of writing as such are negligible in comparison with the changes due to other factors, such as the general 'style' of the period, the shifts of intellectual and spiritual interests, or the influence of the scripts of other nations under whose cultural sway the author or scribe was living." (p. 84).

What is that "art of writing?" Simply the act of writing itself.

[2] This is how he defines the word *ductus*. It appears to me very undesirable to introduce this term into English, for instead of clarifying concepts, it confuses them. Even in German it is not a precise palaeographical term, and is rather uncommon, at that, denoting vaguely "character, style."

And what is meant by its "inner development?" Presumably the changes which are caused by the act itself of writing.

How is it possible to describe as negligible a factor whose decisive importance is self-evident? To neglect it would be like denying the importance of the phonetic factor in the development of a language. (This comparison has been chosen not as a mere picture but as a real parallel.)

The first two of T.'s three causes melt into one, because "the shifts of intellectual and spiritual interests" would, according to him, cause a change in "the general 'style' of the period."

How does this cause work? We would like him to give us something more tangible than words—actual examples: photos and charts of real letters in their changing forms to illustrate in detail how "the shifts of intellectual and spiritual interests" effected changes in the Hebrew script of various periods. And we would like similar documentation in support of the second of his causes.

Pre-Christian Material Only?

Another of T.'s "reasons for perplexity" is that my "method of dating consists of a comparison of the script of the Scrolls with material belonging, or reputed to belong, to the pre-Christian period. This simply begs the question; for, by limiting the terms of comparison to the pre-Christian material, it is assumed that the Scrolls are pre-Christian, too." (p. 81).

When writing down this argument he seems to have overlooked that he himself quotes (on p. 82, lines 13, 17 = p. 83, line 11) my actual words "first century C. E." as the date of some of my comparative material.

Material From After the First Century C. E.

Quoting my statement that it is "obvious that the writing of Is[aiah Scroll *A*] is considerably older than anything we have after the first Christian century"[3] he declares: "From the point of view of method such a generic statement is obviously insufficient." (p. 81).

Not so very obviously, perhaps. There was no need to extend my examination beyond the 1st cent. C. E. because if the Cave Scrolls had contained forms which do not occur in our material, and which could only have been explained as developments from the 1st cent. forms, I should have been forced to conclude that the script was later than the 1st cent. C. E.

In other words: If, by my examination I have shown that the script of those three Scrolls is indubitably older than the 1st cent. C. E., then it is *a fortiori* older than any later century, and no comparison with later material is needed.

But something which in the case of mathematics, geology, etc. would be a matter of course is not so when it comes to Hebrew palaeography. As it is not an old-established branch of research, scholars in other fields feel they cannot judge for themselves who is right and who is wrong in the "palaeographical" controversy, i. e., they cannot decide (1) who among the protagonists has done many years of specialised research in

[3] BASOR 113 (1949): 33.

palaeography itself; (2) who of them is at least in touch with it; and (3) who is without either qualification, even though, in pursuing other studies, he may have handled MSS. or inscriptions all his life. Small wonder then if many scholars did not realise that they were not spectators in a controversy between palaeographers, but of an attack on palaeography by certain specialists in other fields, who, for various reasons, felt impelled to enter the palaeographical arena, even though they had not the necessary equipment, thereby befogging the issues.

It was for this reason that I decided to include in my Palestine Exploration Fund lecture of March, 1950, comparative material from later times—from the 3rd to the 11th cent.[4]—ludicrously superfluous from the palaeographical point of view though it might be.

Measuring Evolution

T.'s question, "*how much older* than the earliest post-Christian specimens of writing is the writing of the Isaiah Scroll supposed to be" (p. 81) was answered by me several years before he asked it. My result was: the script of Isaiah Scroll *A* antedates the beginning of the Ossuary period by about a century and a half.[5] From which it follows that the Scroll is about four centuries older than Dura, etc.

A few lines lower down, however, T. shows that he knows my answer, for he says: "The propounders of the pre-Christian date for the script of the Scrolls ought to show that four centuries must separate this script from that of the Dura Europos fragment and seven centuries from the script of the other manuscripts. They ought also to show that the 'evolutionary line' of the development of the shape of the letters precludes the placing of the script of the Scrolls within the first Christian centuries." (p. 81).

Does this imply that the palaeographer who bases his work on the "evolutionary line" should, if his method were correct, be able to measure mechanically backwards or forwards from a fixed point—so and so much "evolution" per century—until he arrives at the time of the script in question? It seems unnecessary to say that only T.'s imaginary palaeographer, using the alleged "evolutionary line" would have such a nice slide-rule among his instruments. In the world of reality, however, if the palaeographer has only one fixed point, instead of two termini, for a given document, he hardly ventures to say that the writing on it looks a couple of generations older or younger than his dated specimen.

Referring to BASOR 113 (1949): 35, where I sum up the results of my examination of Isaiah Scroll *A*, and which he quotes, T. says: ". . . Unfortunately, Dr. Birnbaum has not disclosed on the basis of what calculation he has reached this particular date." (p. 83).

My summary, however, does not start with the passage which he has quoted but with the preceding paragraph, where I show that the date of the Scroll must be somewhere between 200 and 50 B. C. E. I then give a list of features indicating an early date within this period, and of features pointing the other way. The quantitative relationship between

[4] *VT* 1 (1951): 98-101.
[5] BASOR 113 (1949): 35.

the two sets of features is then expressed numerically.[6] Presuming that I need not work out for my readers this simple proportion, I provided the data and gave the result.

T. finds that my method " rests on the assumption that the number of occurences of late forms stands in linear relation to time elapsed."

T. uses mathematical terminology. But palaeography is not mathematics. To satisfy our practical requirements, we generally need no more than approximate results. The degree of approximation depends on the amount of material available, and on its distribution in time. If we can ascertain the date to within a half century, we may be well satisfied. When there is a fair amount of material, more or less evenly spread throughout the period in question—as in the case of the comparative material utilised for the Cave Scrolls—then such a result is attainable, and even somewhat greater precision may sometimes be achieved.

Our method is the only possible one, and the one in general use.

Co-Existence of " Early " and " Late " Forms

The presence of relatively " earlier " and " later " forms (of different letters) in one and the same document has to be explained by T. by the following theory: " Should we not rather say—still remaining on Dr. Birnbaum's ground—that the presence of several late forms in the script of the Scroll indicates that the writing is relatively late and that the earlier forms are . . . survivals? " (p. 83).

From these words it would appear that T. has never followed up closely the development of a script. Otherwise he would have noticed that at each particular moment there are—in standard writing as well as in cursive hands—" early " and " late " forms, side by side. For every letter develops at its own pace—although occasionally groupings do occur—so that every form may be said to be a " survival " among " late " forms, and a newcomer among " old " forms.

If T. had made a detailed and systematic study of palaeography his " cursory glance at the result achieved by Dr. Birnbaum's method of dating " would not " reveal the paradox that is involved in it." (p. 83). What is this paradox? " It postulates that late forms of letters which we encounter for the first time in a set of ' datable ' objects are really more than a century older than this earliest date at which they are found." (p. 83).

Does he mean that the late form of, say, *'ayin* in the Isaiah Scroll is " really more than a century older than " the Scroll itself? Why is it a paradox that a form found in a certain document, which happens to be the earliest we have, should already have been in existence a considerable time before its occurrence in that document?

How To Use One's Own Method

" But let us take a step further," says T., " and concede that the method used by the sponsors of a pre-Christian date is absolutely correct. Even so, it seems that their own premises should have led them to the

[6] There was, unfortunately, an obvious slip: instead of " half as many " it must, of course, read: " a third as many," as shown by my result there.

conclusion that the script of the Scrolls examined by them is much later." (p. 82).

In support of this contention we would expect, from the term "premise" and the plural form in which it is used, that a number of important points of a general nature would follow,—that he is about to disclose a sweeping fault on the part of the "sponsors."

But all we are given is a single detail of a single letter (*M*).

From the point of view of method this is obviously insufficient (to quote one of T.'s expressions). We would not expect him, within the compass of an article, to produce pages upon pages showing for every letter of the alphabet that on the basis of my method the Scrolls are much later, but if he had worked it out systematically for himself—as his readers are entitled to expect—he would certainly have told us so.

However, even his one solitary example not only fails to prove his point but actually proves that the opposite is true. He quotes from my discussion of the final *M* in the Uzziah Tablet that the left-hand downstroke is severed from the top bar (Fig. 2, line 6). Having found in the Isaiah Scroll "many cases [where] the left-hand downstroke is severed from the bottom bar" (p. 82), he evidently sees in this form (Fig. 2, line 3) an analogy with the form brought by me as a proof of lateness (because the severance of the downstroke from the top bar is a further step in the development). "If this is so, do not the forms of *mem*, final and non-final, in the Isaiah Scroll represent a still further step in the development'?" he asks. (p. 82).

Fig. 2.
Development of Final *M*[5]*

This argument signifies that he took the existence of a gap in the *M* to constitute the essence of my proof. I have not said anything of the kind, of course. What matters, is not the existence of a gap but the place where it occurs: on top it is a sign of lateness; at the bottom, of earliness. The explanation is very simple: we have "to take into account the manner in which the scribe formed the letter." Originally an oblique stroke started above the top stroke and crossed it (Fig. 2, lines 1-4). At the next stage it did not start from above but *at* the top stroke (*ibid.*, line 5). Finally it did not even touch it but started lower down (*ibid.*, line 6). Hence when the final *M* has a gap on top it is younger than one without this gap.

The cross stroke was originally short (*ibid.*, line 1), became longer in the course of time, changed its direction somewhat (*ibid.*, lines 2-3), and finally joined the left-hand end of the base (*ibid.*, lines 4-6). Hence a final *M* where there is a gap at the bottom is older than one where there is no such gap.

The absence of the top gap and the occasional presence of the bottom gap show that Isaiah Scroll *A* is older than the Uzziah Tablet. Thus instead of demonstrating that my own method should have led me to a later date, T. has managed to *support* my proof of an earlier one.

He then goes on to explain in what order the non-final *M* was written (p. 82), but does not say how that should affect the dating of the Scroll.

Next he declares my description of *M* in the Habakkuk Scroll to be incorrect, because, he says, it takes no account of the manner in which the scribe formed the letter. (p. 82).

Fig. 3. The structure of *M* in the Habakkuk Scroll. Line 1: Real structure. Line 2: Erroneous reconstruction.

My description did take that manner into account: "The top part is a tick. From its point a straight, occassionally curved, stroke slants to the middle of the line height. From its right end a straight—rarely a wavy—stroke slants to the line bottom. Here . . ." [7] (Fig 3, line 1.) T. completely misunderstood the structure of the letter. He was under the mistaken impression that the scribe wrote what are really two independent strokes, the left-hand part of the tick and the left-hand downstroke, "by the same movement of the *calamus*." I have drawn T.'s construction in Fig. 3, line 2. A more systematic knowledge would have saved him from the error of reading a mediaeval cursive structure of the letter into the Habakkuk forms.

"Spurious"

His discussion of my method, T. finally declares, "leads to the conclusion that the claim of an early date based on a spurious method must be rejected." (p. 84).

His discussion was based on wrong premises and assumptions, and carried out without the necessary palaeographical knowledge. Hence his final judgement in these matters can carry no weight.

It might have occurred to him that my results for all three Scrolls fall within the same period and yet at different times. This should have caused him to ask himself what was the reason for such a coincidence. Each of the three MSS. was examined by me on its own, in a separate article, so that a wrong method on my part would have yielded quite different results. The fact that there was identity of period for all three MSS., while the dates varied, should have served as a hint to T. that my method was, perhaps, after all, not so "spurious."

There is another point which ought to have made T. more wary. How is it that the results arrived at by the application of this method tally so well with the findings of the archaeologists? I had no chance to be influenced, consciously or subconsciously, by their opinions. My palaeographical dates were published before the jar fragments had even been found in the Cave, let alone before the archaeological experts had expressed their views, and before the publications containing them reached me.

And finally, should not the circumstance that my dating goes very well with the results of scholars who base their pre-Christian dates on internal evidence, have made him stop to think? He speaks in his introductory remarks of the need for "a thorough critical examination

[7] *JBL* 68 (1949): 165.

of all the arguments adduced in the discussion" (p. 67). Why, then, did he not devote a chapter to their arguments—where he would have had the advantage of being on his own ground—and try to dispose of them, in the same way as he did in regard to the mediaeval theories?

"A Very Strong Presumption"

The sentence quoted by me from p. 84 of T.'s article goes on: "and that there is a very strong presumption in favour of the view that at least some of the examined Scrolls fall within the Christian period."

Now, even if he had proved that my method was wrong and my results incorrect—how would such a proof lead to that "very strong presumption?" There is not the slightest connection between these two things. If it is proved that an object is other than white then it has not thereby been shown to be green: "*a* is not *b*" does not mean "*a* is *c*."

Diverse Hints By Teicher for the Guidance of the Palaeographer

Referring to the forthcoming publication of the photographs of the Scrolls, T. mentions two things which the palaeographer will have to avoid: "unfounded assumptions like that of the 'evolution of the shape of letters' or concepts like that of the 'early Hebrew book-hand.' This concept has a definite meaning in relation to contemporary cursive writing: it has no meaning in the absence of such writing." (p. 84).

"Evolution" has been discussed above. We shall add here a word about the "concept . . . of the 'early Hebrew book-hand'."

If T.'s term "absence" signifies that we happen to have no specimens of cursive writing contemporaneous with the Leviticus Fragments, then his objection to the term book-hand would be very strange. For why should that prevent us from calling the script used in the writing of a book a book-hand? By the same reasoning we would not be allowed to call the script in the Samaria and Lachish Ostraca, what it, of course, is, a cursive.[8]

He goes on to add some words of advice for the guidance of the palaeographer in his task of dating MSS.: "Hebrew palaeographers may also remember that script is an aspect of the general civilization in a given historical period. It reflects, though on a smaller scale, the 'style' and tendency of its period." (p. 84).

There is a short answer to this "reminder": What practical assistance does this idea give us in dating the Cave Scrolls?

Obviously none whatsoever.

T. next tells us that the "contents of the manuscripts, the circumstances of their discovery, the external indications of their origin, the history of their owners, etc., are again important elements for fixing the date of the script" (p. 84)—which is not exactly novel information for a palaeographer.[9]

Nor is his subsequent sentence: "A reference set of manuscripts with

[8] It seems hardly possible that the term "absence" can mean here "non-existence." For as we have plenty of cursive material from Samaria and Lachish it would be absurd to assume that after their time cursive was no longer in use.

[9] A palaeographer has, however, to deal in the first place with the script.

well-established dates is an extremely valuable, and in most cases a very expeditious, means for fixing the age of undated scripts." (p. 84).

This is rather like telling a philologist to make sure he uses grammars and dictionaries, or a mathematician that tables are a great help. For such a "reference set of manuscripts" is the palaeographer's principal means of fixing dates. In terms of numbers, in Hebrew palaeography, such a set means a systematic collection of thousands of photos.

By the way, as to the idea of "expeditious": exact comparison never is, even at best, an expeditious, let alone a "very expeditious," procedure. It is always a difficult, slow and laborious task.

T. then continues: "but great care must be exercised in using this means, for it may happen that the date in the reference-manuscript is mistaken, or forged, or copied from a much older original." (p. 84).

This again is a commonplace.

Without doubt, the "dating of manuscripts is . . . a very complex procedure; it has indeed all the complexity of an historical investigation" (p. 84). But like an historical investigation, it must start from a solid basis.

The Tetragrammatic Edifice

Such a basis of palaeographical knowledge, in place of *a priori* theories, would have made impossible T.'s fantastic statement that Isaiah Scroll *A* is later than the Habakkuk Scroll and the *El* Fragment. Not having at his disposal the palaeographical method—which proves beyond doubt that, whatever might be their precise dates, Isaiah Scroll *A* is considerably older than those two documents—he is obliged to arrive at his result by conclusions drawn from a literary reference. He deduces from Origen's well-known words about the use of the Palaeo-Hebrew script for the Tetragrammaton that the old alphabet was no longer employed in his time, and that therefore the Hab. Scr. and the *El* Fgm.—where that script does occur—must be older than the Isaiah Scroll where it does not. There is no need to discuss this argument as such, or whether there is any force in it, for the factual premises on which it is based are wrong. He refers to "Origen's words that in his days the fashion in accurate Hebrew manuscripts of writing the Tetragrammaton in ancient Hebrew characters had died out." (p. 85).

Now, Origen did not say that.

Here are Origen's words: "in the more exact of the copies the name is given in Hebrew characters, not in the Hebrew ones of the present day, however, but in the ancient ones." [10]

Origen is not speaking of Hebrew but of Greek MSS. Not only is this a matter of course, in view of the grammar, but it follows also from the words 'Hebrew characters,' which would be an illogical expression if the whole MS. were written in Hebrew characters.

In Jerome we actually have the word "Greek": "we find the four-lettered name of the Lord given in ancient letters, in certain Greek scrolls, up to the present day." [11]

[10] *en toîs akribestérois dè tôn antigráphōn hebraíois charaktērsi keîtai tò ónoma hebraïkoîs dè oú toîs nūn allà toîs archaiotátois* (*Commentary to Ps.* 2.2).

[11] "nomen Domini tetragrammaton in quibusdam graecis voluminibus usque hodie antiquis expressum litteris invenimus" (*Prologus Galatus*: Migne 28: 594).

And such specimens are in existence,[12] while Biblical MSS. in Hebrew Square script, with the Tetragrammaton in Palaeo-Hebrew characters, are not to be found in any collection, and there is nowhere the slightest hint that they ever existed.

The evidence of the Cave Scrolls might indicate that such MSS. never existed: Isaiah Scroll *A* has the Tetragrammaton in the same Square letters as the rest of the text because it is a Biblical MS., whilst the Hab. Scr. uses the Palaeo-Hebrew for it, as do the Hodayot Scroll and the *El* Fgm. for the word *'l*—just because they are *not* Biblical MSS. Prof M. H. Segal has pointed out the reason: ". . . so that the sacred name in Square script should not make the scroll sacred, since the Palaeo-Hebrew script was at that time considered to be a profane script, as is said in the Mishna, (Yadayim, ch. 4, m. 5): '[Palaeo-] Hebrew script does not make the hands levitically impure,' i. e., it has no sacredness whatsoever."[13] In other words: if a non-Biblical MS. were to contain the Tetragrammaton in Square letters, it would, through that, receive the status of a Biblical MS., i. e., it would become sacred, and problems of levitical purity would arise for its users. To obviate that, the Palaeo-Hebrew script was used.

Origen's statement combined with the evidence of three bits of Roman pottery found in the Cave, create for T. another " very strong presumption," this time " in favour of the third century C. E. as the date of the Isaiah Scroll." (p. 86).

As Origen was speaking not of Hebrew but of Greek MSS. T.'s whole Tetragrammatic edifice collapses, and his " very strong presumption " cannot be based on the Palaeo-Hebrew letters within the Square text of some of the Cave Scrolls.

Pre-Christian Tetragrammata

An additional argument adduced by him for the Christian period is " a significant, although entirely negative, circumstance," namely that " no Tetragrammaton in ancient Hebrew characters, as far as I know, has up to now been found in a pre-Christian manuscript " (p. 86). This, being an *argumentum e silentio*, is by definition very weak. And that is not all. The use of such an argument is understandable when the absence of one thing is contrasted with an abundance of the other thing,—in the present case: absence in pre-Christian times, abundance in Christian times. The abundance however consists of two cases.[14] Is this a good enough basis, in the circumstances, for an *argumentum e silentio?*

[12] See p. 51, No. 13*, and C. Wessely, *SPPK* 11 (1911): 114.

[13] *Ht'm lzh hw', kdy hšm hqdwš bktb 'šwry l' yqdš 't hmgyltḥ, ky hktb h'bry hqdwm nḥšb'z lktb ḥwrl,kmw š'mrw bmšnh ydym d.h.: " ktb 'bry 'ynw mṯm' 't hydym " klwmr 'yn lw kl qdwšh.* (*'rṣ Yśr'l. Mḥqrym bydy't h'rṣ w'tyqwtyh.* vol. 1 [1951]: 39.

[14] See note 12.

"Everything"

After inserting a linguistic paragraph just before the end of his palaeographical chapter,[15] T. says: "In conclusion, everything so far supports an early Christian date for the Scrolls" (p. 86). If we look through the pages up to this sentence we find that this "everything" consists—apart from the Origen passage which he had misunderstood—of only one point: the Roman objects. There is not a word in this chapter about what he considers to be the age of the script, while in his preliminary chapter all he says is that the writing "is older than that of the papyri or of any other known Hebrew manuscript." (p. 69).[16]

It seems that he has been so carried away by his efforts at disproving my method and dates that he has finally persuaded himself that he has thereby done a great deal to prove his own date. But he has only repeated what he said on a former page, and "*a* is not *b*" still does not mean "*a* is *c*."

[15] He refers to the "peculiar orthography of the Isaiah Scroll, the substitution of synonyms for other expressions contained in the M.T. Isaiah and other instances of scribal interference with the text" as suggesting that it "was written down in an atmosphere of spoken Hebrew" (p. 86). A detailed and exhaustive examination of the text would first have to be made before one could venture to entertain even remotely, such a possibility, which contradicts a considerable body of clear and unambiguous evidence about the linguistic conditions in the last few centuries before the Christian era—not to speak of the 3rd cent. C.E., his date for our Scroll.

And as far as the spelling of the Scrolls is concerned, it would be interesting to hear how it could indicate "an atmosphere of spoken Hebrew." After all, even a language which is only written and not spoken, is in general not read with the eyes only but articulated, e. g., in private study, private prayer, public worship. Hence those who write or copy a text are concerned with the visual rendering of something audible. And so an atmosphere of "pronounced Hebrew" was as natural at that time as at any earlier or later period.

The spelling of the Scrolls is, by the way, not "peculiar." It just differs from that of the M.T. (and only little, for that matter). Something is not peculiar merely because we do not happen to be used to it.

It is interesting to note that the conclusion drawn by others from the *plene* spelling of the Scrolls is exactly opposite to T.'s: that a much diminished knowledge of Hebrew made more help in the reading of the consonantal text necessary.

But however all that might be, it has not "some bearing on the problem of the age of the [Isaiah] Scroll" (p. 86) if, by that, T. means that it helps to establish the date. In what way? Even if we assumed that he was right in thinking that the Scrolls were written when Hebrew was still spoken –this would still not yield a 3rd century date. We would then have to conclude, from what we know of the history of the Hebrew language, that the Scrolls must belong to the 5th-3rd cent. B.C.E., which is, of course, impossible.

If T. did in fact intend this point to corroborate his date, then it would be a good example of a circle. His real starting point ("very strong presumption") is, of course, the 3rd cent. C.E. date, which he has reached from a different basis. In other words, his argument would mean: As the language and spelling of Isaiah Scroll *A* suggest that it was copied out at a time when Hebrew was still a spoken language, and as the Scroll dates from the 3rd cent. C.E., Hebrew was still a spoken language at that period. As Hebrew was still a spoken language in the 3rd cent. C.E., the fact that the language and spelling of the Scroll suggest a date when the language was still spoken, supports the 3rd century date of the Scroll.

[16] He adds: "excluding from this statement the Dura Europos fragment; the relation of its script to that of the Scroll itself still awaits proper investigation" (p. 70). Cf. below, pp. 37-39, 45-46, Figg. 13, 16, 18.

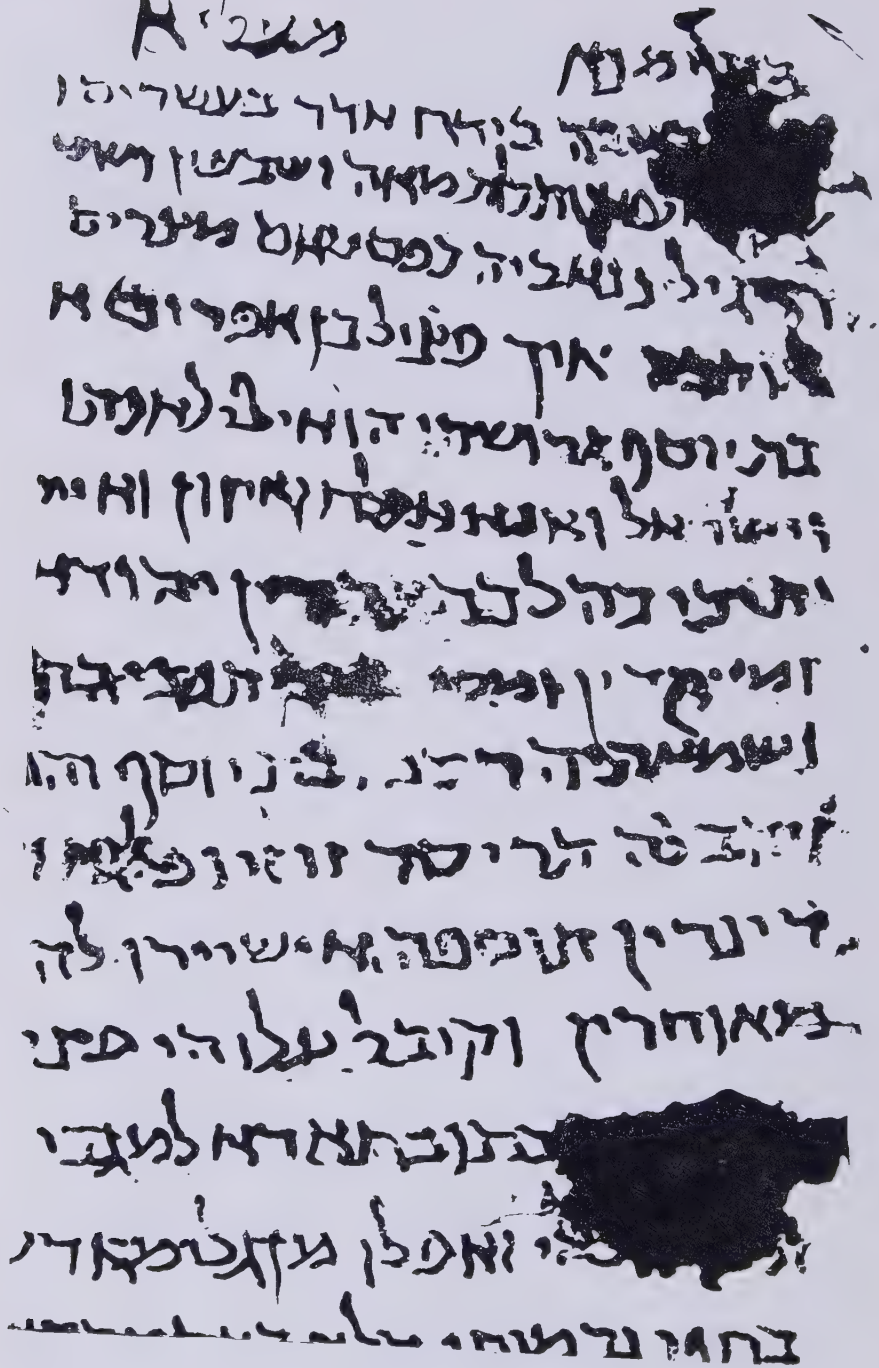

Fig. 4a.

Complete absence of Cave Scroll influence. (I)
Marriage deed of 1065.[6*]

Fig. 4b.

Isaiah Scroll.[7*]

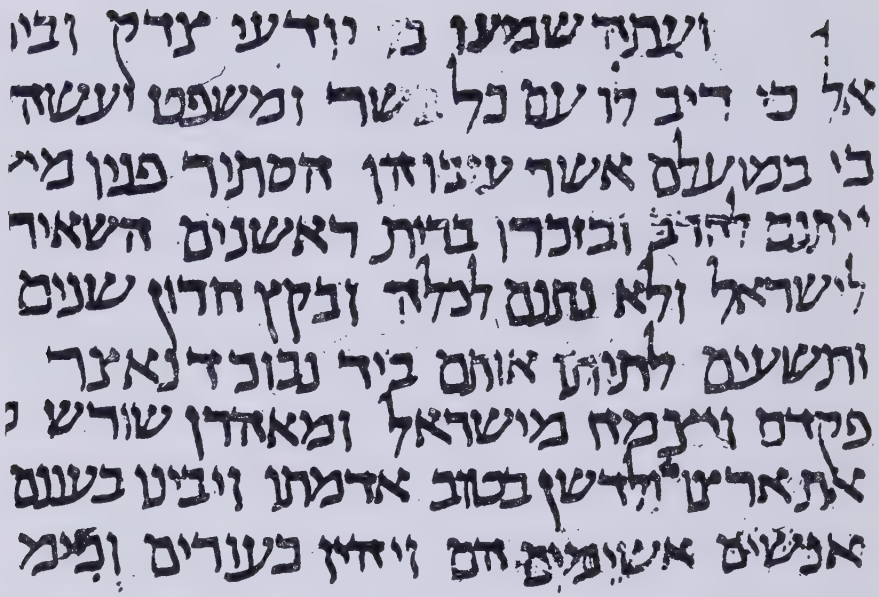

Fig. 5a.

Complete absence of Cave Scroll influence. (II)
Damascus Covenant, Fragment A. (Square script).[8a*]

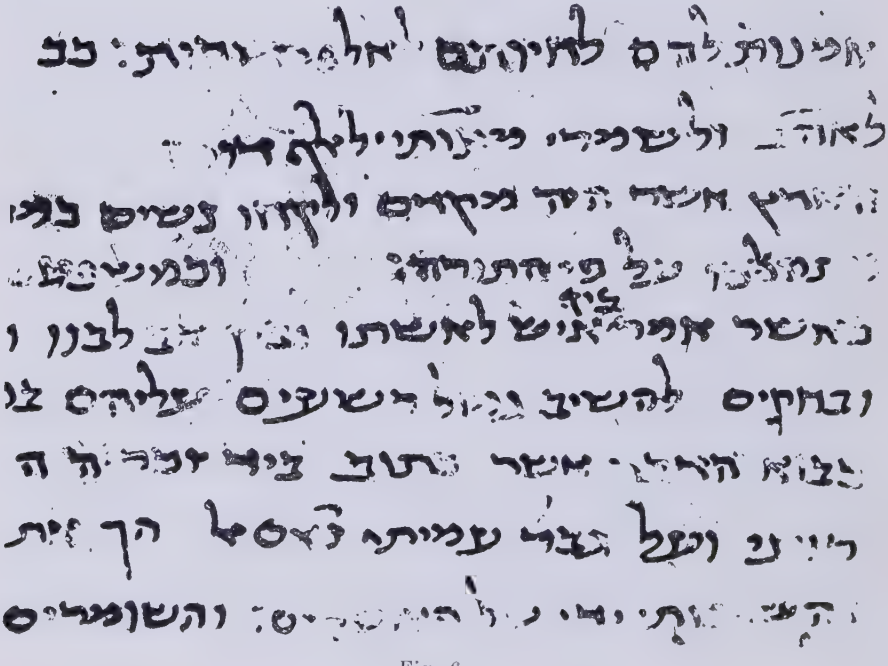

Fig. 6a.

Complete absence of Cave Scroll influence. (III)
Damascus Covenant, Fragment B. (Cursive script).[8b*]

Fig. 5b.

Isaiah Scroll A.[7]*

Fig. 6b.

Covenant Scroll.[9]*

Another example of how unfortunate it was for T. to devote so much attention to the palaeographical aspect appears in a later section. Here he says: "If one surveys the history of the Hebrew script . . ." (p. 90). That sounds as if a quite considerable amount of well-worked-out material were available to him for such a survey—while all there is at present is the raw material itself: the MSS. and inscriptions.

But let us hear the rest of the sentence: "If one surveys the history of the Hebrew script one cannot fail to notice an unusual and at first sight inexplicable phenomenon." And what is that?—"The characteristic forms of letters known now to us from the 'Ain Feshkha Scrolls, particularly the dilated final *mem*, are on the whole not represented in the Hebrew papyri . . . which extend to the beginning of the ninth century. However, these forms appear suddenly in Genizah manuscripts of the tenth or eleventh centuries, either sporadically in single instances or pervasively, as, for example, in the Marriage Contract of 1076, written in Fustat." ("Ms. B.M. 8663").

It is a thousand pities that he has not told us which letters, apart from the final *M*, in his opinion answer to this description. And as for the documents containing them he has mentioned only the deed of 1065 [17] and the two fragments of the *Damascus Covenant*, but not a single MS. with "sporadic instances."

As it happens, in 1935 I drew an alphabet from that deed (Fig. 7, line 3). But nothing unusual struck me. Neither in this nor in any other 11th cent. document on which I was working, did I notice any sudden break in the development, affording evidence of a new and inexplicable influence.

And now that T. brings an "unusual . . . phenomenon" to our notice, I confess I cannot see it, and when he speaks of the script of those three documents as being clearly an artificial imitation of the shape of the letters in the 'Ain Feshkha Scrolls" (p. 90), one can only gasp in amazement. One look at Figures 4 to 10 should convince anyone better than words could do of the enormity of such an assertion.

There is nowhere a "dilated final *mem*" in these medieval documents, nor any other feature showing reversion to the forms of the Cave Scrolls. The medieval forms are not imitations of the Cave Scroll script, they do not suddenly appear after the Cave discovery of the 9th cent., but simply follow on the forms from before their own period (Fig. 9).

And is not the script of the Marriage deed of 1065 (and of the *Damascus Covenant* fragments) identical in type with that of the 1050 deed? (Fig. 10). If so, T.'s characterisation of the latter must also apply to the former. And this is what he says regarding the writing of the Cave Scrolls and of the 1050 deed: "there is not the slightest resemblance between the two . . . they are utterly different" (p. 69).

His "renascence of the old forms of script" (p. 90) is a baseless fantasy.

[17] T.'s figure, 1076, is due to a slip,—the original date is 1376 S.E.

Fig. 8.

Complete absence of Cave Scroll influence. (V) Column *a*: *Damascus Covenant*, Fragment A. (Square script).[8a*] Col. *b*: *Dam. Cov.*, Fragm. B. (Cursive script).[8b*] Col. *c*: Isaiah Scroll *A*.[7*]

Fig. 9.

Absence of break between the script of 8th cent papyri and that of the 106 deed.
Column *a*: Marriage deed o 1065.[6*] Col. *b*: Liturgica fragm., 8th cent.[10*]

Fig. 10. Identical types of script.
Column *a*: Marriage deed of 1065.[6*] Col. *b*: Witness deposition of 1050.

Fig. 7. Complete absence of Cave Scroll influence. (IV)

III

In dealing with the article by O. H. Lehmann, entitled "Materials Concerning the Dating of the Dead Sea Scrolls: I: Habakkuk"[1] we shall first of all turn our attention to the section treating of "traces of a partial vocalisation."

Vowel Points

Le. believes he "may perhaps [have] discovered" some vowel signs, which, "though used irregularly," "seem to point partly to the Tiberian, partly to superlinear vocalisation" (p. 51). He refers to this as only a "suggestion . . . the confirmation of which depends on an examination of the original, as dots in MSS may be due also to causes of various kinds" (*ibid.*). There is, however, no need to leave the question open. All that is required is a careful look at the printed plates.

The positions of the dots in relation to the letters, as well as their sizes, shapes and colour intensity, vary so greatly that they cannot possibly be other than wholly accidental. There are also dots in open spaces, nowhere near a letter. How account for them? It should be mentioned, too, that some of the dots are not where Le. describes them to be,[2] a fact which vitiates his readings, and that some of the "signs" are by no means too happily described by him.[3] For these reasons alone his suggestion that the dots may possibly be vowel signs cannot receive acceptance.

In addition some questions of a general nature present themselves:

Out of the thousands of vowels in our Hab. Scr. is it likely that only about a dozen would have been so marked?

Is it at all likely that one and the same vowel sign should have been arbitrarily placed now over and now under a letter? (p. 52, Nos. 3 and 5.)

Is it not very improbable that one and the same sign—a dot—should have been made to serve for three different vowels: *o*, *i*, and *a*? (p. 51, No. 2; p. 52, Nos. 4 & 5.)

The Tetragrammaton

Another way of approach to the problem of dating made by Le. is "through an examination of the tetragrammaton" (p. 33). By this he evidently means that because the Hab. Scr. has a Palaeo-Hebrew Tetragrammaton, it must be of the same period as certain extant documents which also contain a Tetragrammaton.

He adduces much material to show the importance of the Tetragr. until right up in the 10th cent. or so. He proffers two sets of material promiscuously—that concerning the Tetragr. in the Palaeo-Hebrew[4] and that concerning it in the Square script. The latter has, however, no relevance for the dating of the Hab. Scr., because here the Tetragr. is written in the old characters.

[1] *PEQ* 83 (1951): 32-54.

[2] E. g., col. V, 14: dot not over the *M* but over the *H*; col. XI, 1: not over the *D* but over the *ayin*; col. XII, 8: not over the *ayin* but over the *š*.

[3] E. g., col. III, 9 over the *D* there are not two dots but a little vertical stroke; col. XII, 14: over the *š* there are not two dots but a little horizontal stroke.

[4] To quote Waddell's Papyrus together with the Aquila Palimpsest as a proof of the correctness of Jerome's statement regarding the Palaeo-Hebrew Tetragr. (p. 46) is a mistake. In Waddell's Pap. the word is written in the new script. Furthermore, this document does not belong to Christian times ("third century"). Its writing has been dated by Greek palaeographers as 2nd or 1st cent. B. C. E.

But what does even the other material contribute towards solving our problem? The extant fragments comprise Greek texts with Hebrew Tetragrammata, and our literary references, too, deal exclusively with the same situation (cf. above, p. 25 f.). By what rules of logic does it follow from this evidence that there must have been in existence (or at least: that it was likely that there were) Hebrew scrolls, containing Biblical books written in Square script, and having the Tetragr. in Palaeo-Hebrew characters? Much less is it possible to go another step farther and, from what is merely an assumption, conclude that the Hab. Scr. must be contemporaneous with our Greek fragments.[5]

The Habakkuk Tetragrammaton

Le. says of the *H* and *Y* of the Hab. Scr.: "Their close affinity to the letters of the Greek Tetragrammaton in the Aquila palimpsests (here influence of Greek uncials!) is surprisingly great" (p. 47). His own drawings directly contradict this statement.

That *W* and *Y* in the Grecian Tetragr. are totally unlike the corresponding letters in the Hab. Scr., may be seen in Fig. 11. We can go even

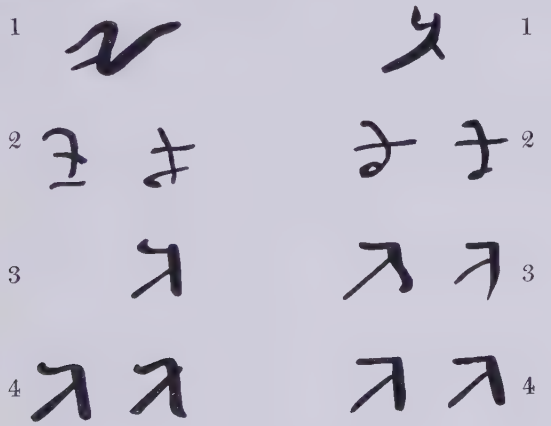

Fig. 11.

Palaeo-Hebrew Forms—Real (1) and Grecian (2-4).
From Tetragrammata. Line 1: Habakkuk Scroll.
Line 2: 3rd-4th cent. C. E. papyrus.[12*] Lines 3-4:
Second half of the 5th cent. C. E. parchment.[13*]

further and say they are not recognisable as Hebrew letters at all. In addition: *W* and *Y* are practically identical. In the Hab. Scr., however, there is no similarity whatsoever between *W* and *Y*, the reason being that they are still constructed in the original way. It is not difficult to reconstruct the stages of the development they may have undergone. The changes were clearly of a more far-reaching nature in *W*, as it assimilated towards the form of *Y*, until finally nothing remained of its original structure.

[5] Le. advances an interesting theory to explain the presence of the Tetragr. in the Greek texts. It would lead too far to show here why his theory is, however, untenable. Cf. above p. 26.

Of the *H* and *Y* in the Hab. Scr., Le. says: " the tendency of simplification and adaptation to the graphic requirements of a late time is marked " (p. 47).

What were these requirements? A statement such as this needed elucidation.

As a matter of fact, there is no reason to look for special circumstances. If, as Le. believes, the script of the Hab. Scr. is already early medieval, very great changes might be expected in the forms between the times of the Lachish Ostraca and Hab., or even between the Maccabaean period and Hab. And even the real period of the Hab. Scr., the 1st cent. B. C. E., is distant enough from Lachish for us to expect considerable changes. They are presumably smaller than they would have been if the scribes had not stopped writing Palaeo-Hebrew MSS. centuries previously: such occasional Tetragrammata as did occur in non-Biblical scrolls did not constitute enough material for a great deal of development to take place.

Le. thinks that the " simplified *waw* in S. H. [Hab. Scr.] seems to stand halfway between the *waw* in the second century coins and the Grecised type in the papyrus [6] and palimpsests " (p. 47).

Fig. 12 shows that an interchange of the positions of lines 2 and 3 would lead to an absurd result. Hence the Hab. *W* cannot be derived from the 2nd cent. coin forms. It can, however, be derived without difficulty from the form known to us from the Lachish Ostraca (*ibid.*, line 1). As a matter of fact, its structure is still the same, although the proportions of the parts have changed. Hence its position must be between Lachish and the coins.

The idea that the Palaeo-Hebrew forms in the Hab. Scr. are younger than the coins, and are not far from the time of the Aqula Palimpsest (late fifth cent.) strikes one as just another of the fantastic assertions that have been thrown up during the Cave Scroll battle.

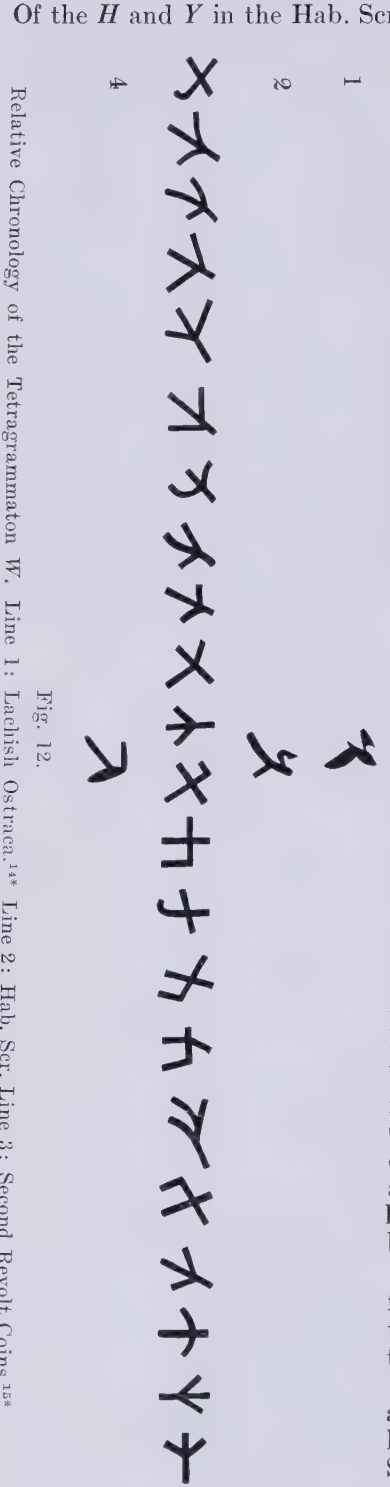

Fig. 12.

Relative Chronology of the Tetragrammaton *W*. Line 1: Lachish Ostraca.[14*] Line 2: Hab. Scr. Line 3: Second Revolt Coins.[15*] Line 4: Aquila Palimpsest.[12*]

[6] In so far as this refers to Waddell's Pap. it is not relevant to our question (see above, p. 34, n. 4).

Triangular Square

Turning now to Le.'s discussion of the Square script of the Hab. Scr., we find he describes it as a " triangular . . . type of Square script " (p. 32).

If we look at a column, it does not strike us as containing many triangular outlines. Examining the alphabet (Fig. 13, lines 1-2), one finds only three or four letters which are perhaps slightly more triangular than even the modern ones: *T* and *S*, which are letters of rare occurrence, *P*, which is not too frequent either, and finally, *Š*, and that is less triangular than in the modern Ashkenazic script of Torah scrolls.

Similarly, he uses the expression " ' triangular ' ductus." Is this " ductus " [7] just another word for what he first calls " type," and is it more or less identical with " style "? We might conclude that from a passage where he says it " seems likely " that the " triangular " . . . " ductus represents the style of a school of scribes or a limited area and period " (p. 35), and that it is therefore a " secondary," not a " characteristic " or " essential " feature. He then goes on to say that a " marked difference of ductus " need not " mean a great difference of period as well " (p. 35).

In other words: Faced with the fact that there is a great difference between the forms of the Hab. Scr. and the rest of his comparative material,[8] he labels the difference as " ductus," declares " ductus " to be an unessential feature—and with that imagines that he has explained away what is unmistakably clear from the forms themselves, viz., that the script of the Hab. Scr. is much older than that of his material. If instead of asserting that the " distinction between characteristic and secondary features . . . has not so far been sufficiently taken into account " (p. 35) he had attempted to work this out, it would perhaps have become clear to him that such theoretical statements do not help us in the least with the dating of our Scroll.

The Dura Script

Le. begins his palaeographical examination by listing some " typical " differences between the writing of the Liturgical Fragment from Dura-Europos, on the one hand, and the script of the Cave Scrolls and the papyri from the Christian era, on the other. This is to show that the Dura Fragment " belongs to a different group of MSS. altogether " from the Hab. Scr. and those papyri, his purpose being to remove the Dura Fgm. as a *terminus ad quem* for the Hab. Scr.

Let us examine in what way the differences indicated by him are " typical " and, according to Le, make it impossible to use the Hab. Scr., the Dura Fgm. and the papyri from the Christian era for comparison (Fig. 13).

G: The Dura Fgm., Le. says, has " a sharply pointed foot." But it is not alone in that; it shares the acute angle at the foot with the papyri. *D* is described by him as having " a curved cap." The top part is, however, not a curve, although its angle is slightly rounded. In line 4 this

[7] Cf. above, p. 18, n. 2.

[8] He tells us that the script of two or three of the Bodleian Geniza fragments have an analogous " ductus " but he has not given us their reference Nos.

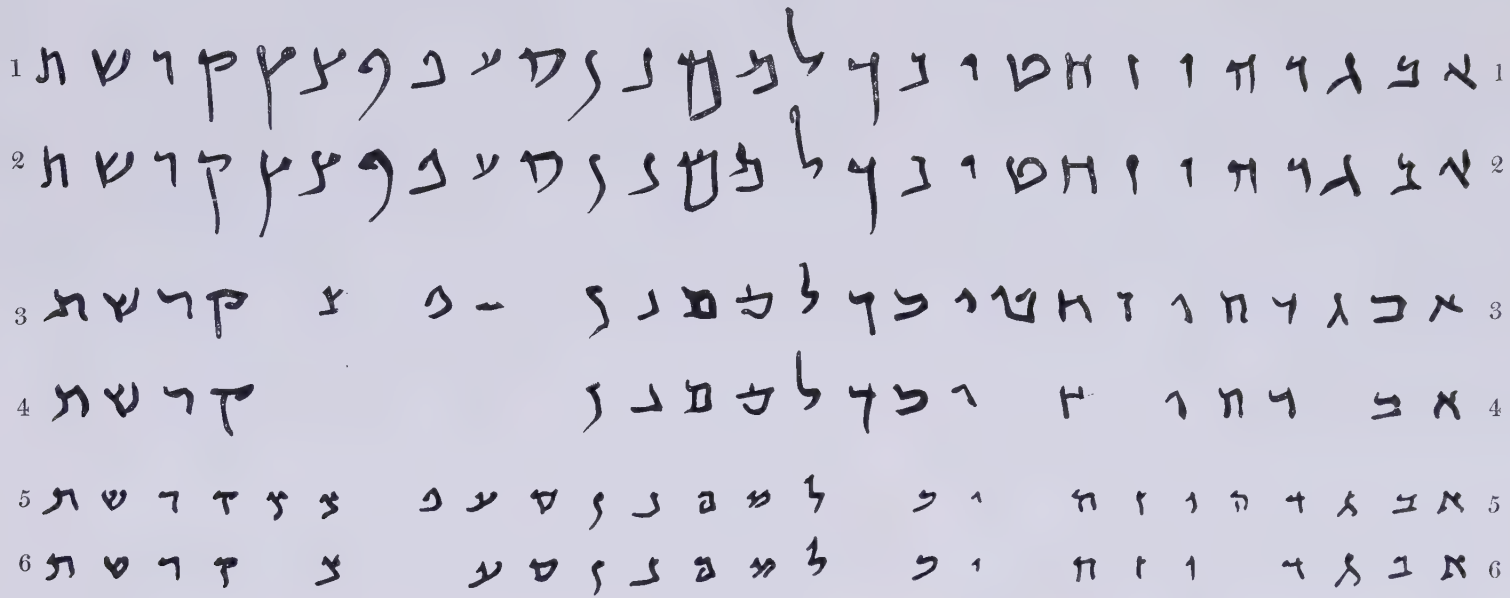

Fig. 13.

Position of the Dura Script

Three alphabets: 1/2: Hab. Scr. 3/4: Liturgical fragment from Dura.[16*] 5/6: Liturgical 4th cent. fragment.[17*]

whole part is clearly a bar, like that in the Pap. Between *H* and *Ḥ*, we are told, there is a distinction, in contrast with the forms in the other groups. But in the Dura Fgm. the two letters are also quite different: in *H* the top stroke reaches the top of the right-hand downstroke, in *Ḥ* the two verticals are connected by a line, which, even when in a slanting position, does not reach the top. *Z*, we are told, " has hardly got a head." Le. has unquestioningly followed the erroneous reading of the first publications: *mzwn*. The error has been caused by the similarity between the third letter of this word and the modern *W*. But a look at all the undoubted *W*'s of the Fgm. will show that this confusion ought never to have arisen. *T*: Forms where the end of the right-hand curve becomes a base are later quite common (cf. Fig. 8, coll. *a* and *b*). Final *M*, Le. says, " does not reach beneath the bottom of the script," but neither does it do so in the Pap. *Q*, we are told, has " nowhere in our alphabets such a right-hand stroke." Lines 5/6 of Fig. 13, however, contain the identical forms.

Thus it is clear that practically all the features which Le. thinks separate the script of the Dura Fgm. from both that of the Cave Scrolls and of the Papyri of the Christian era, are shared by the Dura Fgm. and the Pap. This is, of course, not surprising, as the interval between Dura and the earliest extant papyri of the Christian era is possibly only half a century, at most a century and a half. We are therefore fully justified in retaining the Dura Fgm. in our comparative material.

The Nash Papyrus

Le. next turns to the Nash Papyrus. Three features [9] are mentioned according to which the Hab. Scr. " seems to be later " (p. 42) than the Nash Pap. (It *is*, of course, later.)

From this relative date he deduces an absolute one: " If following Albright's lower limit, P. N. [Nash Pap.] could perhaps be assigned to the first century B. C.—the liturgical use of the ten commandments in the synagogue ceased in the Christian era—our MS. of S. H. [Hab. Scr.] can no longer belong to that century." (p. 42).

It seems inadmissible to base a deduction on " if " and " perhaps." The result is then just a guess.

In the same way we might choose the 2nd cent. B. C. E., or any earlier one when the Decalogue formed part of the liturgy recited daily by the Priests in the Temple.[10]

The Dating of the Habakkuk Scroll

Looking round for corroboration, he thinks he finds it in the papyri of Christian times. He sees a " close relation in essential features " (p. 43) between these and the Hab. Scr., and analyses " some characteristic

[9] One of them has been erroneously observed: The final *M* of the Hab. Scr. is not —as he indicates—of line height, but extends below the line bottom.

Here he has contradicted his own correct statement of a few lines higher up: that the final *M* in the Dura Fgm. differs from the Hab. one in that it does not protrude beneath the line bottom.

[10] Cf. *Tam.* 5, 1.

resemblances" for about two thirds of the alphabet. From this he draws four conclusions:

The first of these is that "the script of the S. H. seems to be more closely related to the papyri of the Christian era" than to that of the Nash P. (p. 44).

The opposite is true, of course (see Fig. 14). Here the medieval

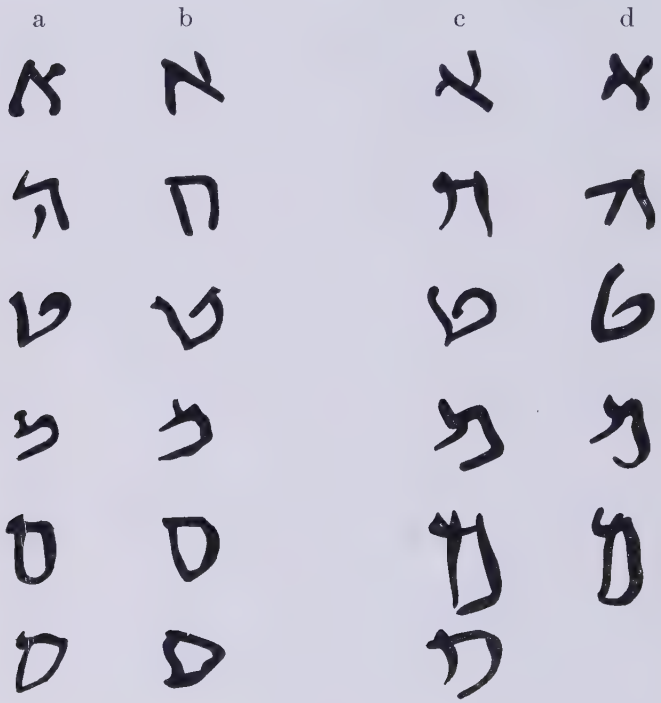

Fig. 14.

Medieval scripts and Pre-Christian scripts contrasted.

Column *a*: Liturgical fragment, 8th cent.[10*] Col. *b*: Letter, 5th cent.[18*] Col. *c*: Hab Scr. col. *d*: Nash Pap.[19*]

columns *a* and *b* (his "St. P." and "B. P.") clearly stand as a group apart from Hab. Scr. and Nash P., which two latter bear a close resemblance to each other.

His second conclusion is that the Nash P. "stands somewhat apart in forms that sometimes are obviously earlier" than those of the Hab. Scr. "but at times appear to be late." The latter contradicts the facts: there are no forms in the Nash P. which are later than those of the Hab. Scr. This may be seen in Figures 14 and 15.

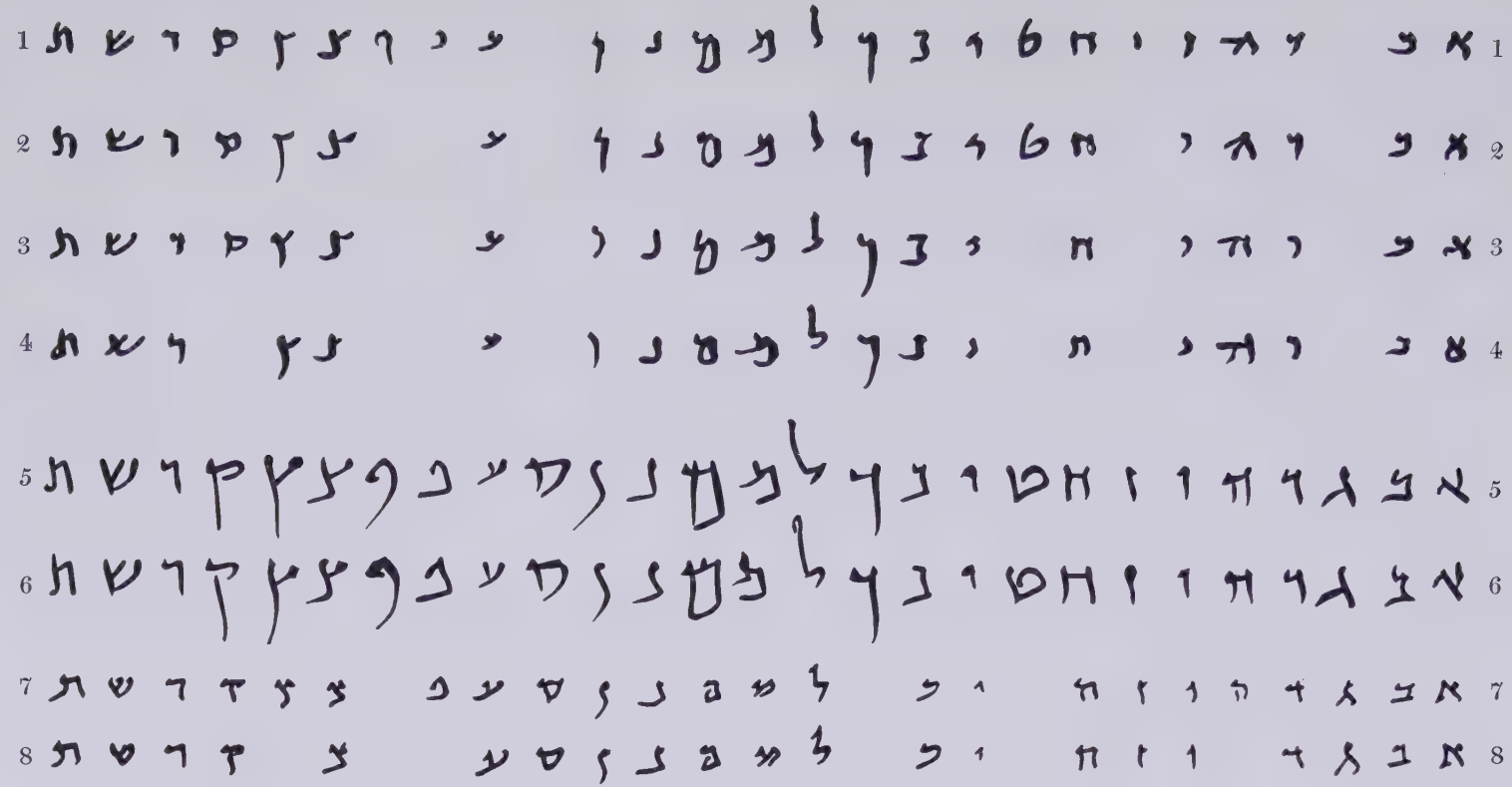

Fig. 15.

Nash Papyrus not intermediate between Hab. Scr. and papyri of the Christian era.

Three alphabets: Lines 1-4: Nash Pap.[19*] Lines 5-6: Hab. Scr. Lines 7-8: Liturgical fragm., 4th cent.[17*]

Thirdly, we are told that "many characteristics" found in the Hab. Scr. "still survived in the seventh-eighth or at least the fifth-sixth centuries A. D." (p. 44). This is identical with conclusion No. 1. We might add that some characteristics have survived even to 1952.

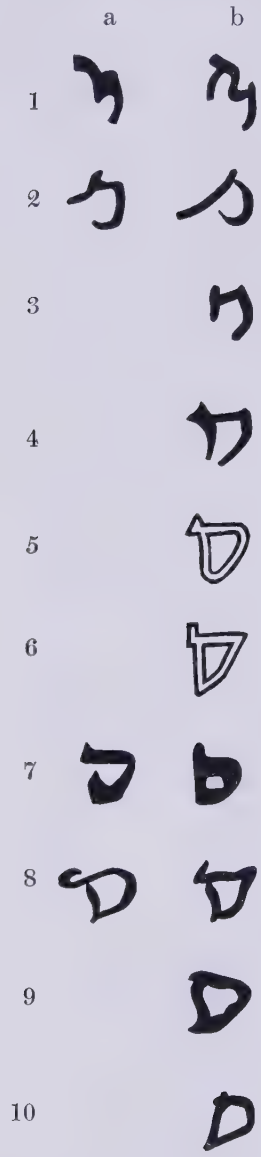

Fig 16.

Thirteen Centuries of Development. Line 1, a: 484 B. C. E.[20*] 1, b: 515 B. C. E.[21*] 2, a: 3rd cent. B. C. E.[22*] 2, b: 3rd cent. B. C. E.[23] 3: Isaiah Scr. 4: Hab. Scr. 5: Between about 37 B. C. E. & 70 C. E.[24*] 6: Second half of 2nd cent. C. E.[25*] 7, a: First half of 3rd cent. C. E.[26*] 7, b: 244 C. E.[16*] 8, a: *c.* 5th cent. C. E.[27*] 8, b: *c.* 4th cent. C. E.[17*] 9: *c.* 5th cent. C. E.[18*] 10: *c.* 8th cent. C. E.[28*]

Fourthly, he concludes that "a number of features point indeed to an earlier rather than a later century of a period between the second-fifth or sixth century A. D." (*ibid.*). But he finally decides against adopting the upper terminus because "there are good reasons supporting the assumption of an archaistic type of script" (p. 44). What these reasons are, he does not, however, tell us. Nor are we given a definition as to what he understands by "archaistic."

His final result, arrived at on the basis of the different sections of his article, is this: "It seems, therefore, probable that our copy of S. H. can hardly be dated much earlier than the beginning of the Masoretic period," and the note is added: "If the texts with old Palestinian vocalisation should be assigned to the fifth century A. D., it may be that the beginnings of the Masora are earlier than has been assumed so far" (p. 54). These cautiously worded sentences, translated into figures, would appear to indicate the 4th or 5th cent. as his earliest possible date for the Hab. Scr.

We have seen from the foregoing and from Figs. 13-15 that this is impossible. In Fig. 16 we have illustrated the chronological position of the Hab. script: it fits in precisely between that of Isaiah Scroll *A* (first half of 2nd cent. B. C. E.) and that of the ossuaries (last third of 1st cent. B. C. E.—70 C. E.). Its time, therefore, must be somewhere within the period 150-37 B. C. E. I have elsewhere [11] dated it as belonging to the first half of the first cent. B. C. E.

[11] *JBL* 68 (1949): 161-8.

IV

In an *Addendum* to an article "A Matter of Method in Hebrew Palaeography,"[1] designed to destroy the basis of Albright's date for the Cave Scrolls by showing that Albright has wrongly dated the Nash Pap., Prof. E. R. Lacheman refers also to my article "The Date of the Isaiah Scroll."[2] He says that it would "take another paper to take up point by point arguments of Trever and Birnbaum" (p. 33) but he omitted to do so.

The Date of the Nash Papyrus

A few words about the date of the Nash P. may be in place here. I did not utilise it in dating the Cave Scrolls.[3] La. did not notice that, or, if he did, failed to realise its implications, for he remarks that I have followed Albright's method. Thus it was not clear to him that his "annihilation" of Albright and Trever had still left me to dispose of.

Being essentially concerned with the negative task of proving our findings to be wrong, La. did not himself attempt to establish systematically the age of the Nash P. But he none the less gave his date for it, the 2nd cent. C. E.

This is untenable. We have palaeographical material from the 1st cent. B. C. E. and C. E.: boundary stones from Gezer and ossuaries from Jerusalem. Comparing the forms of their letters (Fig. 17, col. *a*) with those of the Nash P. (*ibid.*, col *b*), it is clear that these are far less developed. Hence the boundary stones and ossuaries are later than the Nash P.

Now, if La.'s date for it were correct, the stones and oss. would at earliest have to be of the 3rd cent. C. E. But they are of the 1st cent. B. C. E. and C. E. Thus La.'s date for the Nash P. cannot be correct. It must precede the 1st cent. B. C. E.

If we tried to estimate roughly its age we would examine the extent to which the forms differ in the two sets of material. Seeing that this difference is considerable, we would conclude that the time interval between them must be correspondingly great. As La. stresses the slowness with which the script developed, two centuries before the ossuaries should be a conservative estimate to suit his requirements. This would bring us to somewhere about 200 B. C. E.—which is in full accord with my date for the Nash P.: the early 2nd cent. B. C. E.[4]

The Age of the Edfu Documents

In the same way it can be shown that his attempt to lower the date of the 3rd cent. B. C. E. papyri and ostraca cannot be right: "If therefore we judge by the slow development of the fifth and fourth centuries papyri, the Edfu should be much later than Albright surmised and the *Nash* even still later than the Edfu" (p. 31). This however, leads to equally impossible consequences: On the basis of a 2nd cent. C. E. date for the Nash

[1] *JQR* 40 (1949): 15-39.
[2] BASOR 113 (1949): 33-35.
[3] Cf. BASOR 115 (1949): 20.
[4] BASOR 115 (1949): 20.

P., one would be forced to infer, from the difference between the scripts, that Edfu should be placed somewhere in or near the 1st cent. B. C. E. From that century we have the Boundary Stones and part of the Ossuaries, and the forms in these would therefore have to be identical with those of Edfu, or practically so. A look at columns *a* and *c* of Fig. 17, however, reveals that they are quite definitely not, so that the documents in question are not contemporaneous. Hence La.'s statement

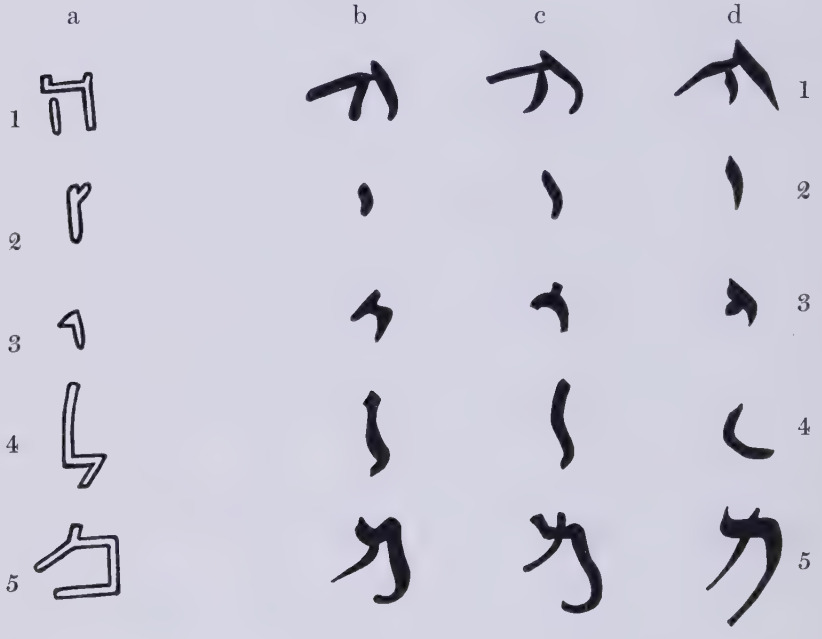

Fig. 17.

Development 400 B. C. E.—70 C. E.

Column *a*: Boundary stones, first third of 1st cent. B. C. E. & ossuaries, between late 1st cent. B. C. E. and 70 C. E.[29*] Col. *b*: Nash Pap.[19*] Col. *c*: 3rd cent. B. C. E. ostraca (Lines 1, 2, 5 [23*]; lines 3, 4 [30*]). Col. *d*: Contract for a loan, 400 B. C. E.[31*]

about the Edfu documents cannot be correct. They must be considerably earlier than the Boundary Stones and Oss.

Let us try to form a rough estimate of their date, too. It is obvious that the forms of Fig. 17, col. *c* (Edfu), are much closer to those of col. *d*, which come from the latest of the dated Elephantine papyri, and are from the year 400 B. C. E. In other words, within the 400 years between columns *a* and *d*, the forms of *c* must be much closer to 400 B. C. E. than to the late 1st cent. B. C. E. (the beginning of the Oss. period). This brings us to the 3rd cent. B. C. E. for Edfu, probably to the early part of the century. A date beyond the middle of the century seems unlikely, being already halfway to the Oss. period.

Free-Hand Drawing or Tracing?

Some of La.'s mistakes are caused by the inaccuracy of his drawings. Because they are tracings, he believes they must inevitably be superior to free-hand drawings.

Tracing, however, is also drawing. It is not as infallible as he imagines. In the present case—whatever the reasons—his chart [5] is unsatisfactory: the forms are not exact, they are stiff, uncharacteristic and sometimes misleading, as the reader may see if he makes a careful comparison between La.'s drawings and the original. Cf. also the following section (*passim*).

The Palaeographical Discussion [6]

ALEPH: La. raises the question as to whether the plain or the looped form is earlier, and decides, after a lengthy discussion about the Nabataean inscriptions, that "the looped *alef* becomes a form of cursive in later Hebrew" (p. 19). It is hard to see why that was needed. The development of the letter is known to us from very early times, and before the period of the Nash P. there was never a "looped" form either in inscriptions or in manuscripts. *B* is *not* "represented by a great variety of forms" (p. 20). I can see only one form, the various specimens differing only slightly, a common feature in informal and cursive scripts. *D* is *not* "like the *rêsh*" (p. 21), although it is sometimes similar. It does not tend to have a top bar like *R*. Nor are *D* and *R* "at times . . . indistinguishable" from the *Y* and *W*. *W* consists of one stroke, *D* and *R* of two parts. It is only rarely that they have the left-hand dot so high up as to give the letter a similarity with *W*. And as far as *Y* is concerned, it is nearly always clearly differentiated from any other letter. *H*: The top bar, whether it is oblique or horizontal, touches the right-hand downstroke near its top. In most of La.'s specimens, however, the top stroke and the right-hand downstroke form a continuous line. This is a form which developed only some centuries after the Nash P. *W*: The last specimen of his drawings from the Dura Fgm. is not a *W* but a *Z* (see above, p. 39). *Z*, we are told, "remains simple," which presumably means: a plain stroke. His drawing from the Dura Fgm. shows the second letter of the word in question, which, as we have seen above (p. 39), is, however, not a *Z* at all. But even if we did not know this, we would not expect a plain stroke in the 3rd cent. C. E., seeing that already early in the 1st cent. B. C. E., in the Gezer boundary stones, the letter had developed a little additional stroke on top. *Ḥ*, he says, "has lost the upper part of the vertical limbs." This is not the case: the connecting horizontal lies under the tops. *Ṭ* does *not* look "more like the Nabataean than any other script." It is paralleled in the 3rd cent. B. C. E. documents and the Tobias inscription. *Y*, we are told, "never develops a full hook." What does he mean? Most of his drawings do show a hook. *K*: he classifies its forms in two groups—one of them, which he describes as "a simple inverted C," I cannot see. The downstroke is a more or

[5] *JQR* 40 (1949): 38-9.

[6] Where not otherwise stated, the forms mentioned in this section are those of the Nash Pap. See Figures 15, 14 d, 17 b.

less wavy line. Where the curving is strongly accentuated, what Albright calls the figure-3 form, develops. *M*: La. says it " has forms exactly like the Dura " documents (p. 22). This is erroneous: what he evidently regards as a single stroke starting from the right-hand top of the letter and slanting to the left, consists of two parts (Fig. 18, line 1): the right-hand one belongs to the top tick of the letter, and the left-hand one is an independent downstroke. The Dura forms (*ibid.*, line 2) are of a much later construction. It can be seen that this is not the only difference between the two sets. *N*: The letter is a wavy line, and sometimes the upper part has a tendency to straighten out. In the Dura Fgm. *N* is a rectangular or acute-angled hook with a sharp point. *P*: the left-hand stroke is short, whereas in Dura it has become long and, on the Tile, both long and curved. La.'s drawing of this letter is particularly unfortunate, and final *P* has been altogether omitted. *Ṣ*, he says, " has only one archaic " specimen. Is he referring to his third specimen? Far from being archaic, this would be a late development. But his tracing is inaccurate. As to the other specimens, their " nearest similarity " is to be found not only in Dura, as he says, but also in the 3rd cent. B. C. E. documents, and that type has remained basically unchanged up to the present day. *Q*: Dura " has more similarities [with the Nash P.] than the Nabataean or Palmyrene," he states. It is, however, much more developed than the Nash form: here the letter is still closed, in Dura it has opened, the right-hand part is mostly angular and pointed, the left-hand downstroke has become very long. The Nash form is closest to the papyri of the 3rd, and to some of the 5th, cent. B. C. E. (and, of course, to the Cave Scrolls). *R*: the development of the top bar is in its first stage, in Dura it is completed—a fact, which La. did not realise. *T*: The more frequent " unlooped " form is not " very much like Dura Heb. Ms.". It is essentially different in construction: the Nash form consists of a left-hand stroke and a right-hand one, in Dura of three parts: a top tick and two downstrokes, one right, one left; the top tick combines the top of the former left-hand stroke with the little curve connecting the downstrokes; the left-hand downstroke issues from the top tick. In other words, the modern structure of the letter has been reached. For the " looped " form La. need not have looked to Nabataean inscriptions—it is present on some ossuaries.

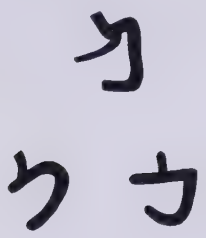

Fig. 18.

Four centuries between two *mems*. Line 1: Nash P.[19*] Line 2: Dura —Tile 244 C.E. (Left).[32*] Liturgical fgm., first half of 3rd cent. C.E. (right).[16*]

" Archaic "

La. makes much of the term " archaic." In the course of his discussion he raises the question as to " what is the difference between an *archaic* form and an *archaizing* one " (p. 26). His answer is: " It seems to me that to be archaic a text must have most if not all of the forms common to that period. A text which has a few archaic forms but a great number of forms characteristic of a much later period is archaizing." If this

simply means that writing containing more late features than early ones is younger than writing containing few or no late features, it is correct—but a truism. What do we gain here by using the term "archaising" instead of "later" or "late"?

La. says: "one does not date a document by the archaizing characters but by the normal forms."

This rule of his is inapplicable because at every time variants of "different age" exist side by side. Some might be used by fewer writers and even by a gradually decreasing circle of users, but as long as they have not altogether disappeared, they still belong to the texture of the script in which they occur, along with the forms employed by a greater number of writers and the "newest" ones. The dating of a MS. must obviously be done on the basis of all its forms. (Cf. above, p. 21).

Origin of the Square Script

La. discusses in some detail uncial MSS., Samaritan uncials, Greek inscriptions, Greek uncial papyri. This is hardly of much help in solving our problems. Although it may be of some advantage to the palaeographer of one script to study the palaeography of another script, on the whole the circumstances are, in each case, so different that each palaeography must have its own methods.

La. employs the word "ductus" without explaining what he means by it.[7] Nor can its meaning be deduced from his way of using it.[8] It is in that connection that he puts forward this suggestion: "the Square script of Hebrew scrolls is the result of a long adaptation to parchment of the square script first used possibly on ossuaries or other lapidary inscriptions." (p. 29).

It is difficult to see how this could be maintained in view of the fact that we have actual evidence of the real course of events: papyri and ostraca, showing the gradual development of the Square script.[9]

The Cave Scrolls

At the end of his article, La. refers briefly to the Cave Scrolls: "Archaisms abound but most of their letters are far beyond the Dura Ms. in evolution" (p. 32). Figs. 13 and 16 illustrate the absurdity of this statement.

As to the age of the Scrolls, he says that "they might be dated at several points in history on palaeographical grounds alone." It would

[7] Cf. above, p. 18, n. 2.

[8] Not much enlightenment can, e. g., be derived from a passage such as this: "The use of the other types of inscriptions, of Herodian time, is irrelevant to the dating of a papyrus. At best they show the origin of the *ductus* for each type of inscription, but since each group may borrow from another group of different geographical origin at any type [time] during the history of the latter group, as Albright has ably shown for the Palmyrene script, this initial use in the latter group does not preclude the use of these forms in the first group at any time in its history, nor in other groups of inscriptions." (p. 31).

[9] Ossuaries cannot be called "lapidary inscriptions," as done by him. The texts on the ossuaries are not inscriptions of the kind used on tombstones of later times but consist mostly of names only, which are not engraved in a lapidary style but are rough graffiti, representations of the contemporaneous cursive, scratched in the soft stone.

have been interesting to learn how this could be done, or at least, to have been told what those points in history are. To my knowledge, a document can be dated to only one point in history. Scantiness of material might have the effect of making this point a rather extended period but that is not the same as several different periods. If the investigator arrives at such a result he can only conclude that he has not brought sufficient knowledge to bear on the material.

In his *Addendum* La. assigns the Scrolls to the Middle Ages. What "finally convinced" (p. 34) him of that date was the fact that the MSS. are ruled. He seems to have taken it for granted that ruling could not be ancient. As regards this point, cf. *VT* 1 (1951): 95-7.

These lines of his are in flat contradiction to a subsequent passage where he speaks of "the injunction of scribes of the first half of the 3rd cent. B. C. E. . . . concerning ruling" (p. 35). He surely does not consider the 3rd cent. C. E. (his "B. C. E." must be a slip) to be medieval?

He actually goes so far as to state that letter forms characteristic of Isaiah Scroll *A* can be found, in the Middle Ages, "everywhere. It does not take long for one who has looked at Hebrew medieval MSS to find these" (*ibid.*).

Maybe "looking" is not enough.

V

"There Is No Misinterpretation on My Side"

The objections raised by Prof. S. Zeitlin against the early date for the Cave Scrolls have been decisively—and without difficulty—refuted by numerous scholars. He, however, believes that by constant repetition he will convince the learned world that in their blindness they are all marching out of step, except himself.

Having told us several times that palaeography is not "scientific," he did not, in his latest article,[1] bother to answer those pages in *VT* [2] which showed why the arguments concerning script which had been brought forward by him in former articles, were untenable. To avoid repetition of what I have already said I shall confine myself here to dealing with a passage [3] in which he aims at disproving my palaeographical date for the Hab. Scr. I have placed my words—which he has misquoted—side by side with his, and have followed this up with the comments of Albright and Burrows, and with Z.'s reply.

(An article [4] by me, devoted purely to the palaeographical aspect, ends as follows: [a] preliminary deduction drawn from the foregoing palaeographical analysis; [b] clear and unequivocal statement of conclusion reached; [c] additional historical remark.)

[1] *JQR* 42 (1951): 133-92.
[2] 1 (1951): 92-4, 98-100.
[3] *JQR* 40 (1949): 77.
[4] *JBL* 68 (1949): 161-8.

Birnbaum: "Our examination has resulted in establishing that most letters in Hab. differ, appreciably to considerably, from their counterparts in the documents utilized for comparison, although they contain elements from all of them.

[*a*] Hence Hab. must have been written in the interval between the early and the late of these documents,—that is, somewhere between 200-50 B. C. E.

[*b*] The following letters show early characteristics: Aleph, Beth, Daleth, Resh. The following, on the other hand, have characteristics of the late period: Gimel, Waw, Zayin, Teth, Kaph, Lamedh, Mem, Final Mem, Nûn, Samekh, Ayin, Pe, Final Pe, Final Sadhe, Qoph, Shîn, Taw. Accordingly it appears that the Habakkuk Cave Scroll was written in the first half of the first century B. C. E., [*c*] with the result that this find has enlarged the range of our knowledge of systematical Bible commentaries by nearly a thousand years."

Zeitlin: "To maintain that the Scroll of Habakkuk was composed during the Maccabaean period somewhere between 200-50 B. C. E., betrays a lack of knowledge of the history of the Jews during the Second Commonwealth. The writing of Commentaries began after the text became unfamiliar to the people. Furthermore, in 200 B. C. E. the book of Habakkuk was not yet canonized. Thus it is actually absurd to assert that the Jews wrote a commentary on Habakkuk before it was even canonized."

Z. omitted to tell his readers my true result, and misled them by arguing against a date which I had not put forward.

Furthermore, those of his readers who may not have happened to read my article, could hardly have failed to imagine from his words that my arguments were concerned with the date when the Hab. Commentary was *composed*, which is totally different from the question as to the date of the script itself.

When Z. published the article in question Prof. Albright took exception, *inter alia*, to the above passage: "Zeitlin goes disconcertingly far in disregard for the statements of different scholars about dating. A few examples. . . . He also attributes (p. 76) to Birnbaum a date for the Habakkuk Commentary 'between 200-50 B. C. E.,' whereas Birnbaum's actual date (*JBL* LXVIII, p. 168) was 'in the first half of the first century B. C. E.' (as Zeitlin would have seen if he had the patience to read the paragraph following the figures he quoted, which represent extreme dates and not Birnbaum's conclusions)."

Z. allowed two years to elapse without reacting to this devastating piece of criticism. Recently, Prof. Burrows of his own accord raised the same issue. Incredible as it may seem, Z. found a reply. As it is illuminating we shall quote it side by side with Burrows's words:

Burrows: "Another example of misinterpretation of another scholar's argument is the statement that Birnbaum dates the Habakkuk Commentary 'somewhere between 200-50 B. C. E.' though in '200 B. C. E.' the Book of Habakkuk was not yet canonized. Professor Zeitlin would hardly say that Habakkuk had not been canonized by 50 B. C. E. He ignores the fact that Birnbaum on the same page narrows the date down to "the first half of the first century B. C. E." [5]

Zeitlin: "Professor Burrows takes me to task because of my misinterpretation of Birnbaum's Habakkuk Commentary. There is no misinterpretation on my side. Dr. Birnbaum stated the Commentary was written between 200-50 B. C. I criticised him for dating it thus, since the book of Habakkuk had not yet been canonized." [6]

No comment is needed.

[5] *JQR* 42 (1951): 109.
[6] *ibid.*, p. 137.

Postscript to Section IV. Together with the proofs of the present publication a new article by La.—"Can Hebrew Palaeography Be Called 'Scientific'?" (JQR 42: 377-385)—has reached me. In it he has, in the main, tried to show how palaeography could be put on a scientific basis by reducing "the human element of error to its minimum." To imagine "that modern photographic techniques can reduce these errors to an almost imperceptible minimum" is to misunderstand the nature of the problem. The real source of error is not the hand but the eye. A striking example is to be found in this very article. Its final passage deals with the construction of *H*, *W* and *H* in the LevFgms. Here with a perfect chart in front of him, La. has completely misinterpreted the evidence of the photo.[1]

It would certainly be an ideal state of affairs if the palaeographer were always able to have at his disposal charts, produced by first having colour photos of the originals made, next having them enlarged to many times the size of the original, then cutting out the single letters, mounting and, finally, rephotographing them on "contrasted material." "This would be costly, it is true, but this would be the only scientific way," La. says. The second half of his sentence is incorrect, and the first is, to say the least, an understatement.

A palaeographer does not have to deal with a couple of MSS. occasionally, but with large numbers of them all the time. What scholar could raise the money to pay for all the photographing, enlarging, cutting out, mounting and rephotographing entailed by La.'s suggested method? We would recommend him to take up Hebrew palaeography himself,[2] and to try and tackle the job of determining the chronology of the Cave Scrolls by this ideal method. As he ascribes the Scrolls to the Middle Ages, his research work would necessitate the production of comparative charts covering about five or six centuries for about two dozen different groups and styles of script, i. e., he would have to build up 2,000-4,000 charts from 6,000-12,000 photographs, enlargements and second photographs, and many hundreds of thousands of cutting-out and mounting processes.

To advocate such a method as the only permissible one is, to put it mildly, somewhat unrealistic for any palaeography. For Hebrew palaeography it is nothing short of fantastic.

To produce clear black forms on a white background is merely a glorified form of the drawing method. Drawing should, however, be quite sufficient to bring out the characteristics of a form. This is perfectly clear from La.'s Chart I—which at the same time shows us that this holds good only if the hand that is drawing is a trained one and if it is governed by a trained and experienced eye.

[1] No further space can be devoted here to charts and a discussion of the details.

[2] His statement that after the 10th cent. there are few dated MSS. in cursive writing is amazing. In my photographic files alone I could show him a very large number of specimens of that kind.

Notes Relating to the Illustrations

1* *SMG* 2: pl. 7.
2* *DSS* 1: coll. 6, 10, 11.
3* *ILN* 1.10. 1950, p. 494, Fig. 6,—J. v. d. Ploeg, *JEOL* 11 (1949/50), Fig 12.
4* R. de Vaux, *RB* 56 (1949): pl. 18.—S. A. Birnbaum, BASOR 118 (1950): 21.
5* Line 1: H. Bauer and B. Meissner, *SBB* 1936: 414.
Line 2: A. H. Sayce & A. Cowley, *PSBA* 29 (1907): 260.
Line 3: *DSS* 1: pl. 32: line 20, etc.
Line 4: *SMG* 2: pl. 8.
Line 5: R. Savignac, *RB* 34 (1925): Fig. 13.—J. v. d. Ploeg, *JEOL* 11 (1949/50): Fig. 23.
Line 6: W. F. Albright, BASOR 44: 8.—E. L. Sukenik, T 2 (1931): 288.—*idem*, *QS* 1931: 217: pl. 1.—W. F. Albright, *Archaeol. of Pal.*, 1949, pl. 26.—J. v. d. Ploeg, *JEOL* 11 (1949/50): Fig. 29.
6* British Museum: MS Or. 8663.
7* *DSS* 1: pl. 51.
8a* Cf. S. Schechter, *Documents of Jewish Sectaries* 1: pl. A (1910).
8b* *Idem*, *ibid.*, pl. B.
9* *DSS* 2: pl. 5.
10* M. Steinschneider, *ZASA* 17 (1879): pl. 7: 1.—D. Chwolson, *Corpus Inscriptionum Hebraicarum*: pl. B: 1.
11* Cambridge University Library: MS. T.-S. 16.79.
12* Osterreichische Nationalbibliothek: Papyrus-Sammlung: Lit. theol. 18.
13* Line 3: Ch. Taylor, *Hebrew-Greek Cairo Genizah Palimpsests* . . . (1900). Appendix: Aquila [Ps. 90 etc.]—*Idem*, *Sayings of the Jewish Fathers* . . . 2nd ed. with . . . a Cairo Fragment of Aquila's Version . . . (1897).
Line 4: F. C. Burkitt, *Fragments of the Books of Kings According to the Translation of Aquila.* (1897).
14* H. Torczyner a. o., *The Lachish Letters* (1938): 76.
15* G. F. Hill, [*British Museum*] *Catalogue of the Greek Coins of Palestine.* (1914).
16* *Excavations at Dura-Europos*, Sixth Season. (1936) Pl. 36: 1.
17 W. F. Petrie, *The Status of the Jews in Egypt* (1922): 38.—*Idem*, *Palestine and Israel* (1934): pl. 16.—P. A. H. de Boer, *VT* 1 (1951): 52.
18* A. Cowley, *JQR* 16 (1903): pl. 2: 5.
19* W. F. Albright, BASOR 115 (1949): 11.
20* E. Sachau, *APO* (1911): pl. 25.—S. R. Driver, *Notes on the Hebrew Text of the Book of Samuel* (1890): pl. 4.
21* See above, note 5, line 1.
22* R. Weill, *REJ* 65 (1913): 16.
23* See above, note 5, line 2.—J. v. d. Ploeg, *JEOL* 11 (1949/50): Fig. 16.
24* E. L. Sukenik, *JPOS* 8 (1928): 113, No. 4.
25* Paris: Musée du Louvre: Cast of part of Kafr Bir'im inscription.
26* F. Cumont, *Fouilles de Doura-Europos* (1926): pl. 108.
27* London: British Museum: MS. Or. 9180 C.
28* Berlin: Ägyptisches Museum: P. 10599.
29* Line 1: Palestine Archaeological Museum: 36.1867.
Line 2: See above, note 5, line 5.—R. A. S. Macalister, *Excavation of Gezer* (1912) 3: pl. 10: 1.—W. R. Taylor, *JPOS* 10 (1930): pl. 2: 3: C and D.
Line 3: See above, note 5, line 6.
Line 4: See here, line 1.
Line 5: See here, line 2.
30* E. Sachau, *APO*: pl. 62: 1: II.
31* *Idem*, *Ibid.*: pl. 34.
32* *Excavations at Dura-Europos*, Sixth Season (1936): pl. 34: 1.

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